AMERICAN
STATISTICAL ASSOCIATION.

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THE SEVENTY-FIFTH ANNIVERSARY CELEBRATION OF THE AMERICAN STATISTICAL ASSOCIATION.

The annual meeting of the American Statistical Association, held in Boston, February 13 and 14, 1914, took the form of a special celebration of the seventy-fifth anniversary of the organization of the Association in this city in 1839. The Association was founded by serious-minded men for a serious purpose, and this celebration was so planned and carried out as to fittingly exemplify the ideals of the founders and the well-established traditions of the Association.

Three sessions were devoted to the discussion of the progress of statistical science and statistical achievement during the period covered by the existence of the Association, and of the present status of such work, particularly in the United States. It would be impossible to adequately present in a brief summary the substance of this discussion or even the conclusions of the authors of the several papers. Some of these papers appear in this number of the QUARTERLY PUBLICATIONS and the rest will be printed in subsequent numbers or in the Memorial Volume* which is to be prepared in commemoration of this event in the history of the Association.

It was a source of gratification to the members of the Association to learn that the Association is not only almost as old as the science of statistics, but that it has had no little part in shaping and promoting the wonderful development of statistical work during that past three quarters of a century.

The meetings were not devoted exclusively to business and serious discussion. Considerable opportunity was afforded for social intercourse among the members and guests at the headquarters, in the Hotel Copley-Plaza, and the final session took the form of a banquet at the Algonquin Club, which was

*See Quarterly Publications of the American Statistical Association, Vol. XIII, p. 559.

attended by a larger number of ladies than had been present at the other sessions.

The extent of the interest aroused by this meeting is manifested in the unusually large attendance of members. There were over one hundred present, many of them coming from a considerable distance. In addition to members the following official representatives of other associations and institutions of learning were in attendance:

F. H. Giddings, American Sociological Society; Columbia University.

F. S. Baldwin, Boston University.

A. W. Whitney, University of California.

F. H. Hankins, Clark University and College.

Frank Sanborn, University of the State of New York.

W. F. Willcox, Cornell University; Royal Statistical Society of England.

D. R. Dewey, Massachusetts Institute of Technology; American Economic Association.

H. Lefavour, Simmons College.

Irving Fisher, Yale University.

W. M. Steuart, United States Department of Commerce.

J. M. Motley, Brown University.

R. S. McCrea, University of Pennsylvania.

F. A. Woods, American Association for the Advancement of Science.

C. H. Verrill, United States Department of Labor.

Herbert B. Dow, American Actuarial Society.

E. S. Macphail, Canadian Census Office.

Ernest H. Godfrey, Canadian Census Office.

Edmund E. Day, Harvard University.

M. L. de Pulligny and M. H. Bunle, Statistical Society of Paris.

Selskar M. Gunn, American Public Health Association.

F. L. Hoffman, American Academy of Political and Social Science; *Deutsche Statistische Gesellschaft.*

Worthington C. Ford, American Historical Society.

Henry C. Metcalf, Tufts College.

Special greetings were received from the following:

The Manchester Statistical Society (founded in 1833), through its president, Theodore Gregory.

The Royal Statistical Society, which had delegated Professor Willcox, one of the fellows, to represent it.

The Statistical Society of Paris, through its secretary, M. Borriol, and which was happily represented by M. Pulligny and M. Bunle.

The *Deutsche Statistische Gesellschaft,* through its secretary, Dr. Würzburger. Being unable to send a special delegate it fitly chose Mr. F. L. Hoffman to represent it.

The Central Statistical Commission of Austria, through its president, Dr. Robert Meyer, who appointed Professor Schumpeter of Vienna, who is now in this country, to represent it. Unfortunately Professor Schumpeter was on his way to the Pacific Coast, but sent flattering messages.

Luigi Bodio, Rome, Italy.

Julius de Vargha, former Director of the Central Statistical Office of Hungary and now Secretary of State.

Professor Julius Mandello, also of Hungary, an honorary member.

Professor C. A. Verijn Stuart, President of the Central Statistical Commission of the Netherlands, on behalf of the Dutch Society for Economics and Statistics.

Verein für Geographie und Statistik, Frankfort am Main, Professor Dr. Deckert, president.

Professor D. Landsberg, President Verband *Deutscher Stadtestatistiker.*

The Board of Directors of the Association elected the following honorary members at a special meeting held on February 14:

AUSTRALIA.

George Handley Knibbs, C. M. G., F. R. A. S., F. S. S., Commonwealth Statistician for Australia.

AUSTRIA.

Dr. Robert Meyer, Privy Councillor; former Minister of Finance; President of the Imperial and Royal Central Statistical Commission.

BELGIUM.

Professor Emile Waxweiler, Director of the Solvay Sociological Institute, Brussels; Professor of Statistics, Economics and Sociology at the University of Brussels; Member of the Royal Academy of Brussels.

CANADA.

Dr. Archibald Blue, Chief Officer of the Census and Statistics Office.

DENMARK.

Professor Harald Ludvig, Westergaard, Professor of Political Economy and Statistics at the University of Copenhagen; author of important books on the theory of statistics and on the principles of mortality and morbidity statistics.

ENGLAND.

Sir J. Athelstane Baines, C. S. I., past President of the Royal Statistical Society; former Census Commissioner under the Government of India.

Major P. J. Craigie, C. B., past President of the Royal Statistical Society; former Secretary-General of the International Statistical Institute; former Director of Statistical Intelligence in the Board of Agriculture.

Arthur Newsholme, M. D., C. B., Medical Officer of the Local Government Board.

FRANCE.

Jacques Bertillon, M. D., former Chief of the Statistical Department of the City of Paris; Member of the Superior Council of Statistics; past President of the Statistical Institute of Paris.

M. Yves Guyot, Member of the Superior Council of Statistics; past President of the Statistical Society of Paris; Ex-minister of Public Works.

M. Lucien March, Director of the *Statistique Générale de la France;* Member of the Superior Council of Statistics; past President of the Statistical Society of Paris.

GERMANY.

Dr. Georg von Mayr, Ex-under Secretary of State for Alsace-Lorraine; former Director of the Royal Statistical Bureau of Bavaria; Honorary Member of the International Statistical Institute; Professor of Statistics, Finance and Political Economy at the University of Munich; President of the *Deutsche Statistische Gesellschaft.*

Dr. Wilhelm Lexis, Professor of Economics and Statistics at the University of Göttingen; former Vice-President of the

International Statistical Institute; *Geheimer Oberregierangsrat.*

Dr. E. Delbrück, President of the Imperial Statistical Office.

HUNGARY.

Dr. Julius de Vargha, Secretary of State; former Director of the Central Statistical Office of Hungary; Member of the Hungarian Academy of Sciences.

ITALY.

Carlo Francesco Ferraris, Professor of Adminstrative Science and Law at the Royal University of Padua; Member of the Superior Council of Statistics for Italy; Ex-minister of Public Works; Member of the Italian Parliment.

Rudolph Benini, Professor of Statistics in the University of Rome; Member of the Superior Council of Statistics.

JAPAN.

Dr. Naosaburo Hanabusa, Director of the General Statistical Bureau of Japan; Vice-President of the Statistical Society of Tokyo.

NETHERLANDS.

C. A. Verijn Stuart, Professor of Political Economy and Statistics at the University; President of the Central Statistical Commission of the Netherlands; Secretary-General of the International Statistical Institute; former Director of the Central Statistical Bureau.

NORWAY.

Andere Nicolai Kiaer, former Director of the Central Statistical Bureau of Norway; Associate of the Statistical Society of Paris.

RUSSIA.

Dr. P. J. Georgievsky, President of the Central Statistical Commission; Privy Councillor.

SWEDEN.

Dr. Gustav Sundbärg, Professor of Statistics at the University of Upsala; Editor of the International Statistical Summaries.

SWITZERLAND.

Dr. Louis Guillaume, Director of the Federal Statistical Bureau; former Secretary of the International Prison Congress.

PROCEEDINGS OF THE SEVENTY-FIFTH ANNUAL MEETING OF THE AMERICAN STATISTICAL ASSOCIATION, BOSTON, MASS., FEBRUARY 13–14, 1914.

The seventy-fifth annual meeting of the American Statistical Association was held in Boston, Mass., February 13–14, 1914. The following program was carried out with some slight changes:

PROGRAM.

First Session, Friday, February 13, at 8 p. m.

1. President's Address, John Koren.
2. The Development and Progress in Statistics during Seventy-five Years, S. N. D. North.
3. Brief Greetings by Invited Guests.

Second Session, Saturday, February 14, at 10 a. m.

1. Coöperation between Academic and Official Statistics, Walter F. Willcox.
2. The Service of Statistics:
   - *a.* To Economics, David Kinley.
   - *b.* To Sociology, F. H. Giddings.
   - *c.* To History, C. H. Hull.
   - *d.* To Biology, Raymond Pearl.

Third Session, Saturday, February 14, at 2.30 p. m.

1. Economic and Social Progress of the United States during Seventy-five Years, Frederick L. Hoffman.
2. The Present Status of Statistical Work and How it Needs to be Developed in the Service:
   - *a.* Of the Federal Government, W. S. Rossiter.
   - *b.* Of States, Adna F. Weber.
   - *c.* Of the Municipalities, F. Spencer Baldwin.
   - *d.* Of Private Societies and Organizations, W. S. Gifford.
3. Annual Business meeting.

Saturday at 7.00 p. m., Banquet at the Algonquin Club, 217 Commonwealth Avenue.

MINUTES.

The meeting was called to order at 4.30 p. m., by President John Koren.

The reports of officers were then called for.

Secretary's Report.

Mr. President and Members of the Association:

I have the following to report in regard to the condition of the Association:

Membership December 28, 1912, date of last annual meeting		634
New members added		93
Deaths during the year	9	
Resignations	31	
Dropped	1	
Total deductions		41
Present membership		686
Net gain		52
Beside members we have:		
Subscribers (mostly libraries)		187
Domestic Exchanges		37
Foreign Exchanges		115
Total mailing list		1,025

The following members have died during the year: John S. Billings, Henry T. Buffington, Robert C. Chapin, George G. Crocker, S. W. Dike, Henry W. Hoole, James MacAlister, H. J. Messenger, and J. B. Sewall.

Four regular numbers of the Quarterly Publications have been issued during the year, containing an aggregate of 329 pages, or an average of 82 pages per number. In addition to these, No. 4, which has been out of print and for which there has been a constant demand, was reprinted. This added about $150 to the cost of printing for the year, but this expenditure has already been justified by the increased sales of early volumes and complete sets.

Four quarterly meetings have been held during the year, as follows: At the Yale Club, New York City, April 17, 1913; at the Ebbitt House, Washington, D. C., April 18, 1913; at the Yale Club, New York City, December 4, 1913; and at the Hotel Westminster, Boston, December 9, 1913. Each of these meetings took the form of a dinner, following which there was a discussion of some single topic. All of these meetings were well attended, and it seems well worth while to continue the

practice of holding such meetings in these three cities, in each of which there are at least one hundred members of the Association.

Respectfully submitted,

CARROLL W. DOTEN,
Secretary.

On motion it was voted to accept the report of the secretary and to make it a part of the published proceedings of the meeting.

TREASURER'S REPORT.

December 24, 1912, to January 1, 1914.

RECEIPTS.

Membership Dues	$962.42	
Sales and Subscriptions	459.95	
Dividends and Interest	198.97	
Balance on hand, December 24, 1912	302.68	
		$1,924.02

EXPENDITURES.

Printing	$872.64	
Postage	74.90	
Salaries and clerical service	409.33	
Expenses	25.20	
	$1,382.07	
Balance on hand, January 1, 1914	541.95	
		$1,924.02

ASSETS.

17 shares B. & A. R. R. Stock @ 188 per share.

S. B. PEARMAIN,
Treasurer.

AUDITORS' REPORT.

We have audited the accounts of the treasurer of the American Statistical Association, for the year December 24, 1912, to January 1, 1914, and counted the securities in his possession. We find the accounts accurately stated and the expenditures properly vouched.

Respectfully submitted,

EDWARD M. HARTWELL,
LEROY D. PEAVEY.
Auditing Committee.

On motion both reports were accepted and ordered on file.

The Committee on Nominations, consisting of Davis R. Dewey, William M. Steuart, and Robert E. Chaddock, reported the following list of officers for the ensuing year: President, John Koren; Vice-Presidents, Charles P. Neill, Edward B. Phelps, Charles F. Gettemy, Charles H. Verrill, Joseph A. Hill; Librarian, Horace G. Wadlin; Treasurer, S. B. Pearmain; Secretary, Carroll W. Doten; Assistant Secretaries, Emil P. Secker, Robert E. Chaddock; Counsellors, S. N. D. North, Frederick L. Hoffman, Walter F. Willcox; Editor, William B. Bailey; Associate Editors, Frederick S. Crum, Warren M. Persons, Julius H. Parmelee, Louis I. Dublin; Committee on Finance, Edwin W. DeLeon, Miles M. Dawson, S. B. Pearmain; Committee on Library, Roger W. Babson, Edmund E. Day, Horace L. Wheeler.

It was voted on motion to favor the movement for the transfer of the function of recording vital statistics in Massachusetts from the Secretary of State to the State Board of Health.

Voted that a delegate be appointed by the president to meet with representatives of the American Society of Mechanical Engineers, to consider standards of graphic presentation of statistics. Leonard P. Ayres was appointed as such delegate

The following resolutions were read and adopted:

WHEREAS, there is great dearth of statistics relating to mortality and disability, due to occupational causes, in the United States;

WHEREAS, such statistics are urgently needed in the immediate present and will be yet more sorely needed in the near future, in order to enable problems of workmen's compensation to be dealt with adequately and economically, and in order to introduce and enforce better means of prevention;

WHEREAS, these statistics would be of the greatest interest and value to members of Congress, heads of departments charged with the enforcement of labor, prevention, or workmen's compensation laws, employers of labor in computing costs and in prevention of accidents, and to all the people;

WHEREAS, for the first time through registration of deaths among a population exceeding one half of the entire population of the nation, and through the taking of a census with due regard to occupation, covering in a separable manner the same population, thus for the first time enabling mortality tables according to occupation to be deduced;

WHEREAS, the special agents, appointed to advise the director of the census what portions of the work projected by his bureau must be dropped because of reduced appropriation therefor, have reported in favor of drop-

ping this investigation, estimated to cost not more than $50,000, because the last to be added to the list in point of time;

WHEREAS, a large amount of money has already been expended upon this compilation in the collection of data, arranging the same, the complete analysis of the deaths by causes, by the sub-bureau of vital statistics and an expensive study of occupational classification, the benefit of all of which will be lost and this invaluable information, now so urgently required, be rendered unavailable until at least after another census will have been taken, unless something now be done; and

WHEREAS, there is now pending in the Senate a resolution offered by Hon. Morris Sheppard, Senator from Texas, calling upon the Commissioner of Labor Statistics to supply for the information of the Senate statistics concerning mortality and disability due to occupation, and the Commissioner of Labor Statistics has reported his willingness to undertake this task, with the coöperation of the Bureau of the Census and of others who have offered data and services; now, therefore, be it

Resolved, That the American Statistical Association, regularly assembled in the City of Boston at this, its seventy-fifth anniversary meeting, deplores and protests against the abandonment, by reason of the short-sighted parsimony of Congress, of the task of ascertaining from all the data available at this time all that can be learned therefrom concerning mortality and disability due to occupation; and further, be it

Resolved, That this Association urges upon the Senate of the United States, as a thing imperatively necessary at this time, the passage of the resolution offered by Senator Sheppard, to the end that information upon these important subjects may be made available as quickly as possible for guidance of the Senators in their consideration of measures for workmen's compensation, public health and prevention of occupational accident and disease, and for the general information of all in authority and of the people of the United States.

It was voted that the president appoint a committee of three members to consider amendments to the constitution of the Association, looking toward the wider participation of members in the nomination and election of officers and in other important matters to come before the Association.

The president appointed the following as such committee: Julius H. Parmelee, Frederick S. Crum, and Carroll W. Doten.

It was moved and carried that the question of further amendments to the constitution regarding fellows be referred to the Board of Directors, to be reported upon at or before the next annual meeting.

The president appointed Leroy D. Peavey and Roswell F. Phelps as an auditing committee for the coming year.

ROBERT E. CHADDOCK,
Acting Secretary.

THE SERVICE OF STATISTICS TO ECONOMICS.*

By David Kinley, PhD., LL.D., *Director of the Courses in Business Organization, University of Illinois.*

On an occasion like this the temptation is great to use the opportunity and privilege of speaking, in extending congratulations to the organization in whose honor this company is gathered, on the long and distinguished service it has rendered to the science for the promotion of which it was founded. Though adding my word of testimony, I must leave to others the more extended presentation of the appreciation of the scientific world of the great service of the American Statistical Association.

A second temptation, to which one is subject on an occasion like this, is to use the few minutes at his disposal in reviewing the work that has been done in the particular field of which he speaks. If I were to do that I would be able, indeed, to paint an encouraging picture of progress in the application of statistics to economic science, and to pay a high tribute to the long list of names distinguished in this particular field of study. This opportunity, however, like the first, I must eschew. It is impossible in twenty minutes to present even a general survey of the history and application of statistics to economics with any hope that it would be intelligently followed even by an audience as appreciative and intellectually keen as a Boston audience is reputed to be.

Nor can I pause, much as I might like to do so, to discuss the nature and limitation of the statistical method in economics, or any phase of its technique. All these things, I must take it for granted, are so well known to you that their discussion would be unnecessary even had I the time to enter upon it. For these reasons I have chosen to confine my remarks in the main to a consideration of the nature of the economic problems which I think can be successfully dealt with by statistical methods, and to point out certain lines of investigation into which I think the statistical method can be pushed in the future with more success than hitherto.

* Paper presented at the seventy-fifth anniversary meeting of the American Statistical Association, Boston, Mass., February 14, 1914.

Statistics began as a social science, or, if you please, the statistical method found its first application in the study of social data. For a long time but little was done or could be done beyond the mere presentation in statistical form of some simple and easily gotten facts of economic life. Consequently, the high hopes with which many students welcomed the introduction of statistics into economic study became dim as the years passed by, and gradually there grew up a feeling that, after all, but little was to be hoped in the way of contributions to economic theory, at any rate, by the statistical method. Of late years the flame of hope has been kindled again, and there is some reason to think that the theoretical discussions carried on a generation ago on philosophical premises may find a new foundation and emerge in more accurate form on the basis of the statistical discussions now going on.

The principal service which, in the past, statistics have rendered and which, therefore, many people have come to think is its only possible service, to economics, is to afford an accurate form of summary statement of economic facts and changes. In other words, statistics enable us to present pictures of existing conditions showing wherein they are defective, according to some accepted standard, and so enable us to make suggestions of remedies for the defects. From this point of view they are simply a statement of facts of interest and importance presented in such a way as to enable the eye and mind to grasp them as a whole more readily and clearly. Their utility, therefore, lies in their adaptability to presentation in a succinct form. If we could present the same facts in the shape of a mathematical formula even a greater advantage would accrue.

In this view the statistical form of presentation is not considered as making any suggestion of relationships in the group or of possible remedies for defects. While I am one of those who think that this is not the whole service of statistics but only the first of a series of great services, and believe that we shall yet found many if not all of our economic theories on statistical data, yet it is true that until now the main result of the gathering of economic statistics has been to enable us to state more clearly the nature of the phenomena which we were studying.

This service of statistics can be readily illustrated by the records of unemployment which have been so generously gathered in the past ten or twenty years. The tabulation of employment or unemployment by trades, for example, reveals the character and extent of disturbances due to seasonal causes, to fashion and other irregular causes, to the character of the industry, and, therefore, throws light upon the obstacles which it is necessary for us to remove in order to reduce unemployment and make employment more continuous. The statistics themselves furnish no suggestion as to the methods of accomplishing this purpose. They simply indicate where and how the remedy, if one can be found, is to be employed. The remedy must be sought in our notions or standards of welfare outside of the statistics themselves. We cannot be sure that the facts presented in the statistical tables, showing for example seasonal unemployment, disclose a causal relation between seasons and unemployment. This conclusion must be reached on other grounds. To put the matter in another way, if our statistics show that at certain recurrent seasons of the year there is a certain amount of unemployment in certain industries, we must inquire into the nature of the industries before we can conclude that the unemployment is due to seasonal causes. To be sure, if we have a considerable series of such statistics on the same industries, and if the same facts are evident at recurrent periods, there is established a strong probability of a causal relation; but a single table of this kind at a given time cannot show this for us, although it may be of great value in enabling us to apply remedies to evil conditions existent at the time. Such remedies must, of course, under these circumstances, be purely empirical and tentative. In the case of the figures of unemployment in a series of tables showing the distribution of labor in the same industries in corresponding seasons, it may be that they will teach us the necessity and perhaps the way of distributing labor and readjusting industries. At any rate they will indicate what to prepare for in particular seasons in the way of extending help to those out of work. In short, the statistics indicate special problems which need attention and solution, or conditions needing specific treatment at the time.

The second service of statistics is to develop relationships of facts within particular groups of data, and lay bases for empirical laws. This service can be rendered to economics by statistics only if the statistics are so numerous that we can be reasonably sure that the facts which they present are representative of the group with which they deal, and so reflect the character of the group as a whole. Moreover, not only must they be thus representative on a particular occasion, but they must be so through a series of occasions. One of the readiest illustrations of what I have in mind is found in the well-known empirical formula sometimes known as Engel's law of expenditure among Prussian families. After investigating a great many budgets of expenditure of Prussian working-men's families, Engel came to the conclusion that certain definite proportions of expenditure of the total income ran through these budgets in a general way. For example, he found that a certain proportion of the total income of the average family went for rent; another somewhat definite proportion for food and shelter, and so on. Now, of course, no one has ever claimed that these proportions were fixed, or that they would be the same in another country, in another social class, or in the same social class at another time. The so-called law, in other words, is a purely empirical formula. It states in ordinary language relationships found to exist in a group of statistical data of a certain character, concerning a certain class, at a certain time, and under certain conditions. The conclusions were of great value, however, not only for purposes of economic description and history, but for showing the condition of the classes with whom they dealt, and revealing the points at which experiments must be made for possible improvement. It is possible to lay down a good many such empirical or positivistic laws for definite economic groups from the statistics already in our possession. Agricultural returns showing relatively decreasing yields for added capital and labor give us the empirical formula sometimes known as the law of agricultural diminishing returns. This particular formula has never been put in so definite arithmetical shape as Engel's law has sometimes been put, yet I can see that it would be possible on a limited area and under certain conditions of tillage to lay down an approximate arithmetically cor-

rect statement of the law under these conditions. Gregory King's famous attempt to establish an arithmetical relation between the changes in the price of wheat and changes in the annual crop is another illustration in kind. Formulas of relationship or empirical laws of this kind could be reasonably drawn only from statistics dealing with small statistical areas.

In the next place, economic statistics enable us to determine the direction or continuity of movement of the group with which they deal. When Agassiz long ago said that, in the biological sciences, "It is the comparative that is significant," he made a statement of equal importance for the statistics of economics. The statistical data must be comparable at different dates, so that a line of continuous movement, if it exists, may be traced through them if they are to serve any purpose other than that of satisfying curiosity, or, if you prefer, intelligent interest.

This service is closely connected with the second service, which I have just described, or that of enabling us to lay down what may be called positivistic or empirical laws of economics. Proper tables of statistics, wages, prices, expenditures, exports and imports, etc., show changes in welfare, indicating progress or retrogression. The important thing in such a series of statistical tables is the continuity of relationships within the group. They show economic movement. In so doing statistics have entered the field of dynamic economics. Such tables at the same time reveal movement and also suggest the nature of the movement and possible explanations of it. In other words we may find in the statistical tables themselves suggestions of the causal relationship of the phenomena presented, so that this service differs materially from that rendered by the kind of tables mentioned in the first instance. For example, tables of wages and prices collected at successive periods enable us not only to compare the condition of the working classes and the prosperity of industries, but may suggest causes or explanations of the changes which the tables reflect. In doing the first they form a basis for economic history. Rogers's History of Agricultural Prices, based on statistical data, and the work of Tooke and Newmarch in their History of Prices illustrate this service. Rogers's work presents figures showing the course of the wages of agricultural labor in England

through a long period. The figures indicate not only changes in the condition of the agricultural laborer, but suggest probable explanations of these changes at different times. For example, when we find in such tables marked changes in the money income of agricultural laborers side by side with corresponding changes in other sources of income through the loss, for instance, of access to common pasturage, turbage, and similar rights, it is not difficult to see a causal connection in the figures themselves, aside from knowledge derived from other sources of political and social changes. To be sure, in a case like this, whatever conclusions we draw can never be entirely safe without being checked by knowledge from outside sources; but the point I am urging is that in the absence of such knowledge the figures themselves serve a good purpose in throwing some light on the changes that are going on.

Still again, economic statistics furnish a basis for legislation and administration. This is a service on which I need not enlarge. Governments the world over are recognizing its importance. The appointment of our own tariff commission is a recognition of this service. The industry with which governments collect and collate tax returns, the insistence of our interstate commerce commission upon proper statistical returns from railroads, and a large number of other illustrations will occur to every one. It is worth while mentioning specifically the latest proposal in this direction, namely, to gather statistics of industry of such character and in such form as to determine what constitutes a unit of maximum efficiency in an industry of a particular kind.

In an inquiry of this kind it would be necessary first to settle from what point of view efficiency is to be defined. Probably it would be efficiency in the sense of determining what is the largest amount of capital that could be employed in a given industry without endangering public welfare by putting industrial control in the hands of the owners of the capital, and at the same time promote public welfare by turning out product at lower costs than could be obtained with less capital, and still more to establish working conditions which would be better than those that could be established in an organization with a less capital. Personally, I believe the inquiry is a practicable one. Certain constants would have to

be assumed and then the problem, statistically and mathematically, would amount to a collection of data to be used in the solution of a problem of maxima and minima.

Another service of statistics to economics is found in the possibility of tracing the effects of economic legislation. Beyond question this is difficult to do because of the multiplicity of influences in any economic operation. Moreover, pretty nearly every law produces results entirely unexpected by its framers and not infrequently of a character opposite to that which is aimed at. Nevertheless, in a general way, and sometimes in particular ways, it is possible through statistical data to trace the effects of particular laws. Inferences based upon such evidence appeal far more strongly to the public mind than do conclusions of the same character based on a priori consideration. The public brushes aside with impatience the opinions of critics of the modern system of workmen's compensation, accident and benefit insurance, and similar legislation, on the ground that these criticisms are *a priori,* theoretical, not based on facts, but on the critics' guesses about human need. When, however, a German authority publishes figures showing that the period of recuperation from accidents and illness under the influence of such legislation has been increased far beyond the normal period required for such recovery, the public is compelled to give attention to the situation.

All sorts of statements have been made about the effects of the reduction of our own tariff in recent months. While it is not true that one man's statement about such matters is as good as that of another, it is true that, if by the collection and proper presentation of statistics after a time we can show in actual figures what some of the results are, attention at least will be arrested. It should be possible to show the extent to which the industries thus less protected are disturbed, whether they have been obliged to curtail their activity, to dismiss some of their workmen, or have met the new situation by greater efficiency, reduced costs, or smaller profits. It should be possible, and certainly interesting, to trace the effect of this legislation on wages, prices and the cost of living; and to show, by figures of industrial establishments, whether the law has caused any new distribution of industry. Certainly tables of

exports and imports ought to throw some light on this last question; for if it be true, as alleged, that we are importing more of the goods which have been in a measure deprived of their former protection, we must pay for them by exporting more goods of some other kind, and it ought to be possible to find statistics which will show whether industries of other kinds have expanded, or whether new industries have been established to furnish the products necessary to meet these larger foreign importations.

But little has been done in the application of statistics to the discovery of economic theory although much has been done in the use of statistics in the way of elucidation. The field, however, is one of great importance, concerning the feasibility of which there still is and always has been a good deal of skepticism. Is it possible to discover a law of interest, a law of wages, or any other general law from the statistics of the subject? By general theory I mean a general expression of the determining forces of a group of economic phenomena, such as wages, or interest, or prices, or the whole field of distribution. Of course, general theory cannot represent accurately "the life and movement of the economic world." It serves to show the general lines of probable truth. In the language of mathematics, a general theory derived from statistical data must be based on averages, and, therefore, must represent a type or limited case. It is the limit towards which actual cases tend indefinitely to approach. This must be so because such a theory must be a law of economic statics. It is conceivable, indeed, that even a dynamic law may be in time discovered from statistical data, but we are yet far from that possibility. However, the pure theory of economic statics has reached the stage where, as Prof. H. L. Moore shows, it can be put in mathematical or symbolic form as a type of the things which that explains.

A use of statistics for the purpose of developing a general theory is possible only by the use of proper mathematical methods. The data must be handled according to mathematical laws. Fortunately, as economic statistics have been developed the mathematical instrument or method for handling groups or masses of data has also been perfected. The calculus of phenomena in masses has been much developed,

especially in the biological sciences in the past ten or fifteen years, and the tool of which Pearson and his colleagues have made such powerful and successful use lies ready to the hand of the student of economic facts.

Since the phenomena are numerous and mutually dependent they can be represented statistically and mathematically only if the statistics are reducible to a series of simultaneous equations, as numerous as the unknown quantities under consideration. It is a mistake to suppose that a series of figures on any one of the subjects of general theory can be treated as a simple equation, or represented graphically as a linear movement. For in dealing with general theory we must use what I have described as the complex homogeneous unit. The data in such a unit are more or less numerous and conflicting. If, however, we are reasonably sure that the character and degree of conflict is approximately constant through a series of successive units, so that, so to speak, the antagonistic elements neutralize one another, so far as concerns the main purpose of our inquiry such units can be used as successfully and logically as if they were homogeneous, in the simple sense of being uniform in character or makeup. One of the most successful illustrations of the use of statistics for the establishment of a general theory is Prof. H. L. Moore's admirable statistical treatment of the law of wages, which established statistically the specific productivity theory, and incidentally checked the deductive processes whereby that theory was reached in the first place.

One hestitates to use an illustration from his own work, yet with apologies for doing so I may be permitted to illustrate what I mean by a complex homogenous unit and the possible deduction of the general theory from statistical data, by a reference to what I laid down some years ago as the main features of what some of my colleagues have been kind enough to call a theory of credit. I used for my purpose the statistics which I obtained in connection with the investigation into the use of credit instruments in business payments, first in 1895 and later in 1903. It is commonly stated that there is no necessary connection between increasing population and volume of business, or, therefore, volume of payments or use of credit facilities. For a very different amount of business may be done

on the basis of the same amount of money and by very different numbers of people. Yet if we can get a complex homogeneous unit, or one in which the other conflicting elements are numerous enough to justify us in supposing that they neutralize one another in the main, while the unit as a whole acts uniformly, I think we may make bold to try to establish some relation between population growth and the amount of money needed and the amount of credit built upon it, and by inference what I have called the static distribution of the precious metals. Not to go into details I calculated the percentage of credit payments for population groups at intervals, and plotted the irregular line which represented it. Smoothing out this line and making it into a better curve by one of the well-known devices, I finally secured what I regarded as a limiting curve from the character of which I made certain inferences as follows:

First, that the proportion of credit increases gradually with population in a country like ours, and under our conditions.

Second, that it increases with more or less regular periodical fluctuations, giving us a series of maxima and minima.

Third, that its rate of increase tends to become less rapid as the volume of population and, therefore, the volume of business grows.

These conclusions I rather jumped at by what may be called a bold imagination from the indications which the curve showed rather than proved by exact induction. When they were suggested to my mind I tried to see whether they could be established deductively, and succeeded, I think, in establishing them by this logical process. Indeed, so much more sure did I feel of the logical argument on account of the paucity of my data, that when I presented the subject some years ago I ignored the statistical presentation altogether, simply because it was so novel at that time and involved so many of what critics would be likely to call guesses, that I felt that the other presentation would appeal to my colleagues more successfully. It is my belief that similar use of carefully collected statistics will yet give us the ground on which we shall be able to formulate some of the general economic theories for which we are groping, and afford formulas wherewith to solve many problems of our industrial life.

THE SERVICE OF STATISTICS TO SOCIOLOGY*

By Franklin H. Giddings, Ph.D., LL.D.,
Professor of Sociology, Columbia University.

Statistics have been of relatively limited service to sociology hitherto because of the quantitative limitations of sociological data.

In economics we have not only frequencies of Sort or Kind, but also abundant frequencies of Size. Prices of commodities, wage rates, interest rates, rents, dividends, and tax rates are measures of size as well as categories of kind. In sociology, while frequencies of size are by no means wanting, as witness, birth and death rates, marriage and divorce rates, by far the greater proportion of our numerical data are frequencies of sort or kind. For example, respective numbers of the different nationalities entering the United States through our ports of immigration, numbers of the foreign and the native born, numbers of the literate and the illiterate, numbers of adherents to the different religious creeds, numbers of persons engaged in various occupations, numbers of the delinquent and the dependent, and so on are frequencies of sort.

Practically all of our statistical operations have been developed through comparisons of size and analyses of size frequencies. Normal frequency distribution, mean square deviation, the coefficient of variability, probable error, and the coefficient of correlation are measures inherent in items of size rather than in items of sort.

Perhaps it has been too hastily assumed that statistical results obtainable through the use of these measures can never be obtained in any other way; and it may be worth while to indicate certain possibilities of statistical measurement that lie in frequencies of sort which, if systematically applied to our large collections of data in census and other reports, might add much to our scientific knowledge of social relations.

Let us then for a moment examine certain statistical measures that may be derived from the mere inequality of sort frequencies; such inequality, for example, as is presented by the num-

* Paper read at the seventy-fifth anniversary meeting of the American Statistical Association, Boston, Mass., February 14, 1914.

bers of foreign-born Irish, foreign-born Germans, foreign-born French Canadians, foreign-born Italians, and so on, in the population of any one of our commonwealths.

The inequality of two numbers a and b is measured by the difference a−b. The total inequality of n numbers, one to another, is the sum of the differences found when each number is subtracted from every other number, and each difference is counted once, or:

Total inequality of n numbers = (a−b)+(a−c) + (a−d) +.....+(a−n) + (b−c) + (b−d) +..... + (b−n) + (c−d) + ... + (c−n) + ... + (d−n).

If the smallest of n numbers be subtracted from the largest, the difference, m−s is the *range* of inequality. It is the quantity of reference from which a measure of tendency to inequality among given numbers—or frequencies—may be derived.

If each lesser frequency in turn be subtracted from a maximum frequency the differences will all be positive. Their sum is the positive inequality of the frequencies in relation to the largest frequency among them, and this, divided by the number of frequencies, is the mean positive inequality.

If each greater frequency in turn be subtracted from a minimum frequency the differences will all be negative. Their sum is the negative inequality of the frequencies in relation to the smallest frequency among them, and this, divided by the number of frequencies, is the mean negative inequality.

If the ascending or descending steps of arrayed frequencies are equal, the positive and negative inequalities, as above defined, are equal. The mean inequality then is equal to one half of the difference between the maximum and the minimum frequency, or to one half of the range of inequality.

If the steps of arrayed frequencies are unequal, the positive inequality will be greater or less than the negative inequality, but the mean of the arithmetical values of the positive and negative averages will equal as before one half of the range of inequality.

The mean inequality of frequencies, of which the positive and the negative inequalities are equal, may be called *iota*, ι. The mean positive inequality of frequencies may then be expressed as ι_1, and the mean negative inequality may be expressed as ι_2.

If the negative inequality of frequencies is equal to their positive inequality, the strength of tendency to equality among them may be described as equal to the strength of tendency among them towards inequality.

If the negative and the positive inequalities of frequencies are unequal, $\iota - \iota_1$ equals $\iota - \iota_2$, but the signs of these differences are opposite. If $\iota - \iota_1$ is positive, it measures strength of tendency towards equality; if negative, it measures strength of tendency towards inequality. If, $\iota - \iota_2$ is negative, it measures strength of tendency towards equality; if positive, it measures strength of tendency towards inequality.

If the negative and the positive inequalities of frequencies are equal $\frac{\iota}{\Sigma f}$ is a coefficient of inequality; if they are approximately equal it is an approximate coefficient of inequality.

In any case, $\frac{\iota - \iota_1}{\Sigma f}$, or $\frac{\iota - \iota_2}{\Sigma f}$ is a precise coefficient of strength of tendency towards equality or towards inequality, according to sign.

These relations stand out sharply in graphic presentation.

If the positive and the negative inequalities of frequencies are equal, the frequencies, when plotted as equi-distant ordinates arrayed, will make a figure that may be bounded at the top by a straight slant line.

If the positive inequality of frequencies exceeds the negative inequality, the frequencies, when plotted as equi-distant ordinates arrayed, will make a figure that may be bounded at the top by a downward curving or concave line.

If the negative inequality of frequencies exceeds the positive inequality the frequencies when plotted as equi-distant ordinates arrayed, will make a figure that may be bounded by an upward curving or convex line.

From mere frequencies of sort we can obtain also a measure of the sociologically important phenomenon of coördination, as distinguished from correlation.

Coördination is equivalence of position. For example, in botanical or in zoölogical classification genera are coördinate one with another, but are subordinate to orders as orders are to classes; species are coördinate one with another, but are subordinate to genera. In the ecclesiastical hierarchy priests are

of coördinate rank, bishops of coördinate higher rank, and archbishops of coördinate rank yet higher.

Superordinated or subordinated coördination, or the coördination of units within each rank throughout a succession of ranks, one above or one below another, is obviously a phenomenon incidental to all subclassification, creating intra-secting categories, or category within category, in descending comprehensiveness.

And when categories are intra-secting, the whole content of category B falls within category A; the whole content of category C falls within category B; and so on.

Therefore, statistically, coördination, superordinate or subordinate, is the appearance of certain same units in each of n categories.

Identity or sameness of content in each of two or more categories may be called Categorical Solidarity.

Since all units of category B, intra-secting category A, occur also in A, it is plain that in these two categories taken together there are as many unit instances of "same content" in more than one category as there are units in category B.

All units of category C, intra-secting B, occur also in B and in A. Therefore, taking the first three categories together, they present as many unit instances of "same content" in more than one category as there are units in B plus the number of units in C.

All units of category D occur also in C, in B, and in A. Therefore, in these four categories taken together there are as many unit instances of "same content" in more than one category as there are units in B, plus units in C, plus units in D.

In general, if categorical solidarity be expressed by S, a comprehensive category by K, and intra-secting categories by k_1, k_2, k_3, ... , k_n

$$S = k_1 + k_2 + k_3 + \ldots . + k_n$$

S, so obtained, is an amount, and it is affected by the number of categories used. The degree, or average density, of solidarity, category with category, may be obtained, therefore by dividing S by n.

If the number of units in each of n categories were the same, and if the "same content" (neither more nor less, nor different) were in each and all categories, K would equal k_1, would equal

k_2, would equal k_3, and so on. Indicating the greatest arithmetically possible solidarity of n categories by G, we of course have $G = K(n-1)$, and the greatest possible degree of solidarity of n categories is $\frac{G}{n}$.

The ratio $\frac{S}{n} : \frac{G}{n}$ or $\frac{S}{G}$ is the coefficient of solidarity for any values of K, k_1 and n.

All measures of solidarity are corresponding measures of coördination.

With these additions to our means of measurement, what are the possibilities of statistical analysis in sociology?

Any association of units presents to the observer certain aspects which admit of quantitative description by statistical methods.

These aspects are: 1, Extent; 2, Duration; 3, Strength; 4, Compositeness; 5, Form; 6, Reaction; 7, Central Point or "center of gravity" of Reaction; 8, Contingency.

The statistical examination of the extent and the duration of association is the simplest of statistical operations. It involves only completeness and accuracy of count, and accurate determinations of date.

Strength of association is resistance to dissolution or disintegration. Dissolution, or disintegration, is statistically measured by the percentage, or other proportion, of associations of a given kind that break up within a given time. Family cohesion, for example, is measured by the divorce rate.

When units of more than one sort are combined in a mixture, the compositeness thereby arising is of three degrees, which may be named respectively, Variegation, Approximate Composition, and True Composition or Heterogeneity.

Variegation is determined by two variable quantities only, namely, (1) the number of sorts in the composition, and (2) the number of items in each sort, that is, the frequencies of the sorts. Differences of magnitude among variants (*i. e.*, units of sort) and the amount of difference that exists between any one sort and any other sort (*i. e.*, inequalities of interval or step) are neglected.

When the categorical or sort frequencies of a composition

are approximately equal, the variegation may be described as uniform.

When sort frequencies are unequal, and one frequency exceeds any other, the variegation thence resulting may be described as modal.

Variegation is measured, and thereby quantitatively described, by the coefficient of tendency towards equality, or towards inequality, as the case may be, of the sort frequencies, of the composition.

Approximate composition takes account of the difference between each frequency and every other frequency in the composition. It is measured by the total inequality of the frequencies.

True composition, or heterogeneity, is the totality of differences in a composition. It includes not only frequencies of sort, and inequalities of frequencies one to another, but also all differences of item from item (in respect of dimension, weight, value, or other magnitude), and all differences of interval or step. If the data are known, heterogeneity can be computed by simple algebraic methods, which are, however, tedious.

Variegation, fortunately the simplest phase of compositeness, is a fact of significance for the organic and social sciences. Easily measured, it is a measure itself, of strength of tendency, or of influences selective or constraining.

Average deviation and standard deviation are assumed to measure the strength of a mode-making tendency, selection, or pressure acting upon variates, *i. e.*, units of size.

If, for example, poppy capsules be gathered at random from a field, and the number of stigmatic bands on each capsule be counted, and the deviation from the mean number be found to be very small, the fact is supposed to tell us that the poppies in that field, or their progenitors, have survived a severe natural selection. The smaller the standard deviation, it is inferred, the greater has been the selective or mode-making pressure.*

The same significance attaches to variegation. The coefficient of tendency to or from equality is a measure of mode-making tendency, selection or pressure for frequencies of sort, probably quite as trustworthy as the coefficient of variability for frequencies of size.

* *Vidi* Karl Pearson, "Grammar of Science," Second Edition, Chapter X, sec. 5.

If seeds of a dozen kinds be planted simultaneously and indiscriminately, but in equal numbers, in a patch of garden, which is then neglected, and six weeks later plants of the dozen kinds are flourishing in approximately equal numbers, kind for kind, we infer that no selective influence has affected them; while if plants of one or two kinds at the end of the six weeks are relatively numerous, of other kinds relatively few, and of the remaining kinds very few, we infer that selection has been rigorous.

Uniformity of variegation then, means a negligible mode-making tendency, selection, or pressure; while marked modality of sort frequencies means a mode-making tendency relatively strong, or a mode-making selection or pressure relatively severe.

The Forms of association are (1) Tangent, or exclusive, no unit of one association occurring in another association; (2) Inter-secting, certain units occurring in more than one association but no association being wholly comprised in another; and (3) Intra-secting, all the units of association B occurring in association A, all the units of association C occurring in association B, and so on in descending comprehensiveness.

Tangent association is otherwise described as "segregation" when the units of each association are similar. The simple statistical problem presented is to count the number of like units that in one or another way are placed or combined in exclusive grouping.

Intra-secting associations are a case of subclassification and coördination, superordinate or subordinate. The coefficient of coördination is a measure of intra-secting association.

The Reactions of Association are measured in units of time, of displacement, and of transformation. Promptness and persistence of reaction are measured in units of time. Degree, extent and amount of reaction are measured in units of displacement or transformation. The statistical description of these reactions involves no unusual developments of method. The difficulties that are encountered arise in the determination of data, in making the original measurements.

The Central Point of Reaction, or, using a figure of speech, the center of gravity of reaction, is that point about which all reactions, including opposing ones, are in equilibrium. If units

react in different ways and with equal power or "weight" the center of reaction is the median of the array of the units. If the several units, either individually or when massed in those squadrons of units which we call frequencies, react with different power or "weight," the center of reaction is found on that side of the median where the heavier weighting occurs. The statistical problems, accordingly, that arise in any attempt to determine the center of gravity of associational reaction are those which in statistical analysis are known as questions of weighting.

No phenomena of society are of greater interest than are the shiftings of the centers of associational reaction. Among these are the shifting of the center of gravity in politics between opposing parties, between radicals and conservatives, between classes and masses, between rationalists and the upholders of instituted authority.

The foregoing aspects of association are of interest in themselves and also, in a higher degree, because of their relation to Contingency. In determining how far association or any phase of it is quantitatively linked with any other fact, we get close to the problems of law and cause. Contingency is measured by a percentage or by the Pearsonian or other coefficient of contingency. Actual contingency when found should be compared with a theoretically probable contingency.

The contingency of any phenomenon of association may be with an extraneous fact, or with any other phenomenon of association itself. Extraneous facts collectively are the environment. The facts of association whose contingencies one with another can be determined are the aspects of association which have here been enumerated.

Among the contingencies of associational phenomena one upon another which admit of statistical determination the following are especially significant.

The strength of association may vary with extent or with duration. It may be found that cohesion increases to a certain determinable point more rapidly than extent increases, and beyond that point less rapidly. For example, as a fact of observation, large states, large towns, large families are generally more coherent than small ones. A similar relation may be found between cohesion and duration.

Again, within limits, the stability of a group may be unaffected by a mobility of its units which permits individual units to disappear from the group and other units from without to replace them. Beyond a determinable limit such mobility of units may impair group stability.

The contingencies of associational phenomena with the degrees and modalities of composition are numerous and highly important.

Within determinable limits similar reactions of associated units are contingent upon other similarities of the units. Like units, in other words, tend to react in like ways. When sorts are combined in a mixture, the units of a sort may react in ways different from the ways in which they react when not in composition. And the effects of composition upon reaction may be a consequence in part of the proportions in which sorts are combined.

Inertia or momentum of associational reaction increases in a determinable ratio with the modality of variegation, that is, as one sort tends to dominate a composition. This is a familiar fact of our social life in all its phases, from fashion to politics. Transformation goes on at an increasing rate, which may be determined, as the proportion of variants from mode increases.

And at this point contingency of associational reaction upon external fact is discovered.

Adaption to environment or circumstance increases modality. Crisis multiplies variants.

Relations of toleration, the reactions of conflict and the reactions of adjustment are notoriously contingent upon forms of association, and these contingencies in a great number of instances admit of quantitative determination. The contingency of toleration is highest when association is tangentail. Conflict is most acute when associations are intersecting. Adjustments, both of the interests of units one with another and between the opposing tendencies towards modality and towards variability, are contingent upon the development of intra-secting association.

THE SERVICE OF STATISTICS TO HISTORY.*

By Charles H. Hull, Ph. D., *Professor of American History, Cornell University.*

When your representative honored me with an invitation to take part in this discussion, I told him (what, indeed, he very well knew) that I had neither statistical experience nor statistical knowledge, and had paid but slight attention to the multifarious discussion upon the relation of history to the newer social disciplines. I could give, therefore, only my personal impressions upon the service of statistics to history, and those but provisionally. He assured me that more was not expected, and I shall not attempt more.

If the second part of this morning's program had been submitted to the founders of the American Statistical Association, it would have perplexed them sorely. Seventy-five years ago the word "economics," as a substitute for political economy (or a criticism upon it) was just making its way into the language, under the dubious aspices of those most unstatistical persons, Thomas Carlyle and Waldo Emerson. The term "biology," recently invented in Germany, had acquired, as yet, no other meaning, among those who knew it in English at all, than that of a particularly vivid form of biography—the story of a man's life as it might be talked rather than written. The hybrid "sociology" was still unbegotten. That statistics should ever serve such strange gods, might well have given the founders pause. Towards history, however, they could have turned with more confidence. Both as a word and as a subject history was, in their day, already venerable, and still respectable; and the service which statistics might render it was, to their minds, entirely clear. In the "Address put forth by the Association at the period of its first establishment,"† its spokesman, the polygraphic Professor Edwards of Andover Theological Seminary, had defined statistics as "the ascertaining and bringing together of those facts which are fitted to

*Paper read at the seventy-fifth anniversary meeting of the American Statistical Association, Boston, Mass., February 14, 1914.

†This address is printed in "Constitutions and By-Laws of the American Statistical Association, with a List of Officers, Fellows and Members. Boston: 1844."

illustrate the conditions and prospects of society." It followed that "every subject in truth forms a part of statistics," and he naturally concluded that the labors of the Association should prove "of inestimable value to the future historian in our own and other lands."* This sweeping conception of statistics, quite in the temper of contemporary Germany practice, was promptly illustrated by the contributions which the Association's secretary, the Rev. Joseph B. Felt, made to the first volume of its "Collections." They are, doubtless, known to many of you, and with them in mind it becomes easy to understand why Schloezer's awkwardly translated dictum† that "statistics is history in a state of progression; statistics are history at a stand," should have met Professor Edwards's full approval, and, as we may infer from Colonel Wright's fondness for the neater version of the same dictum, that "history is past statistics, statistics present history," have become, for a time, almost a part of the Association's creed.

Today, however, the situation is reversed. Regarding the nature and extent of the service which statistics may render to economics, to sociology, or even to biology there is, I fancy, less dispute than now exists regarding the serviceability of statistics for the purposes of the historian. To understand this change we must appreciate that "history" and "statistics" are words of variable and even ambiguous meaning, and must determine in what meanings we will take them for our present purpose.

History is an old word, blurred by careless handling. Of its many meanings two only need engage our attention. Sometimes "history" denotes a method, sometimes a subject. The historical method of ascertaining and presenting past events is, in a general way, familiar to us all. We readily appreciate its applicability to various subjects, and the slightest inquiry into historiography would show that the historical method has, in fact, found most diverse applications. But the interest and importance of its application to the past acts of man as a

*"Constitution," etc., pp. 13, 21, 23.

†*Ibid*, p. 13. Achenwall had written: "die Lehre von der Statsverfassung eines oder mehrerer einzelnen Staten, ist die Statistik." Schloezer, who brought out a seventh enlarged edition of the Abriss (Goettingen, 1790) after Achenwall's death, inserts in the text, after "Statistik," a parenthetical explanatory "(Staatskunde)," and adds in a note: "Staatskunde [d. h. Statistik] ist eine stillstehende Stategeschichte; so wie diese eine fortlaufende Staatskunde." Pt. 1, p. 5.

social being, his wars, arts and industries, his church and state, so far outweigh all other applications of the historical method that, as a subject, history has come to mean a reasoned narrative of man's social doings in the past, or of some of them. Like the dyer's hand, the historian's method is subdued to what it works in; and history as a method has been profoundly affected both by the antiquity of its beginnings, in an age of intellectual naivety and by the human character of the subjects which historians, following in some measure the pattern set by their predecessors, have found themselves called upon to deal with.

Statistics also has two meanings, similarly related. Statistics *is* a scientific method of wide applicability. Statistics *are* a body of facts and inferences—usually but not necessarily social—collected and interpreted according to that method. We have, then, two methods, the historical and the statistical, each, by preference applied, as it happens, to the same or similar matters, producing, the one history as a subject, the other statistics as a subject. The question remains to be answered, whether the two methods are as similar as the subjects to which they are applied.

Statistics is a newer word than history, and its method is even newer than its name. Achenwall, who invented the name in the middle of the eighteenth century, referred it to the Italian word *statista,* a statesman;* and for him, as for his follower Schloezer, statistics meant a general account of contemporary affairs, of the national fabric in widest sense, such as might be of use to a public man: Keltie's "Statesman's Year Book" is a modern English example. Robert Mills's "Statistics of South Carolina" (1826) and George White's "Statistics of Georgia" (1849) are earlier examples from American practice. The statistics of these compilers, which are also the statistics of the American Association "at the period of its first establishment," can scarcely be said to have any conscious method. Their collections were quite miscellaneous, not to say capricious. They did, indeed, show a predeliction for such data as might be expressed "in terms of number, weight and measure."† But they were by no means

*G. Achenwall. Abriss der neuesten Staatswissenschaft der heutigen vornehmsten europaischen Reiche. 1749. Einleitung.

†Sir William Petty. Political Arithmetick. 1690. Preface.

restricted to such, and seldom used them comparatively or in mass measurement.

As a method, then, statistics took its shape within the past seventy-five years, and it fell, consequently, under the influence of those instrumental devices which, as applied by all the natural sciences, have imparted to nineteenth century thought its most pronounced character. The first to apply the familiar method of the natural sciences in the new field of statistics was, perhaps, the Belgian astronomer, Quételet. Instead of concerning himself with such specific details as interested the antiquarian mind of Dr. Felt, Quételet employed the so-called Law of Large Numbers. In order to count and compare the numerous units involved, he arranged them in classes, within each of which all the units, whatever their individual diversities, were assumed to be alike for his enumerative purposes. He thus came to deal in the field of statistics, as all modern sciences do in their several fields, with typical abstractions. These are, no doubt, of great instrumental value for scientific purposes; but they can claim to find no exact counterpart in any real object. None of us has ever met "the statistical man" any more than "the economic man." They are two of the many convenient fictions of science. By the application, then, of scientific method to statistical facts, Quételet began, about the time when this Association was founded, to deduce such general social laws as seemed to him to warrant some measure of quantitative prediction about society as a whole, though he was careful to say* that they implied nothing, of necessity, as to any particular member of society. The measure of his success in what he came to call "social physics"† may have been somewhat less than he anticipated, but it was at least sufficient to make most modern statisticians his conscious or unconscious followers. Statistics had found their method, which is the general method of the natural sciences. It operates, as the sciences all do, with "laws" which apply to aggregates and averages (or other types) but not to the peculiarities of individuals. A rhombohedral crystal of oxide of silicon with trapezohedral tetartohedrism

*Recherches sur la loi de croissance, pp. 1, 2; Recherches sur la penchant au crime, pp. 2, 80.

†Physique sociale, par Ad. Quételet. Bruxelles. 1869. The first version of this book, published in 1834, he called Sur l' homme et le développement de ses facultés, ou essai de physique sociale.

is quartz,—and there you are. It may be large or small, green, purple, yellow, or pink. It is still quartz.

Following statistics, the newer social disciplines, folk-psychology, sociology, and the rest, have developed their methods under the same influence of all-conquering natural science, whose "glory fills the world with loud report." It was inevitable, then, that history, the oldest, of the social disciplines, should be called upon to mend the ancient error of its ways and by installing a modern scientific outfit of general laws and instrumental abstractions, to elevate itself to the rarefied atmosphere where true predictive science dwells. The trumpet of this summons gave no uncertain sound. Comte was confident; Buckle was cock-sure; and the modern materialistic philosophy of history threatens the bourgeois historian with the same extinction in which socialism is presently to engulf all capitalistic institutions, unless he shall straightway forsake his abhorrent, and probably venial pretense of an interest in the deeds and characters of individual men, and shall concern himself solely with the class struggle and the necessary laws of its evolution.

"Evolution," ladies and gentlemen, is the hocus-pocus of the scientific nineteenth century, just as "nature" was the hocus-pocus of the revolutionary eighteenth, and "society" promises to become the hocus-pocus of a sympathetic twentieth. Man, no doubt, will carry on, as he did through preceding centuries.

Meanwhile for the mere historian to object when others, chiefly non-historians, seek, in the sacred name of science, to apply their methods to a subject-matter which he has long dealt with in a manner lamentably unscientific, would be at once arrogant and futile. But without claiming the privilege of such folly, he may perhaps justly ask, he may even be generously allowed, to determine for himself what sort of results he, as a historian, will aim to achieve. And in so far as the methods of natural science may be incapable of reaching that sort of results, he may, for his part, abstain from employing them, not worrying himself unduly about the pains and penalties that shall in consequence be denounced upon him. If, by the use of scientific laws, results superior to his shall be produced, recognition will surely reward the achieve-

ment, and the mere historian will not be the last, let us hope, to acknowledge the new day. Meanwhile, however, we must take history as precedent and practice have shaped it, historians as in consequence they are. The serviceability of the statistical method for the past and present purpose of such persons is the subject of our immediate enquiry.

The majority of intelligent historians (if any historians may be allowed to be intelligent) would agree, I fancy, that the statistical method, being a specific type of the method of natural science, is not their proper method and cannot become their principal tool. For the ultimate units with which the historian deals are not atoms, or any sort of instrumental abstractions, whose individual differences, if any exist, may be ignored, but they are men and the deeds of men. All social phenomena are at bottom human deeds, with qualitative differences, each from each. These it is the characteristic business of the historian to study. Men and the differing deeds of men, as they present themselves for historical contemplation, seem to him too complex and too variously conditioned to be subjected to the concept of general law, as the natural sciences derive that concept from the observation of phenomena assumed to be uniform. He, for his part,can seldom find, and may never assume, that his observation of one man is as significant as his observation of another. He must rather assume the existence among men and their acts of those qualitative differences which are a fundamental fact of all organic life. This involves him in no quarrel with science. Science too freely confesses the qualitative differences of individuals. But for the purposes of science they are overlooked because they are of negligible importance. For the purposes of history, however, their importance is often the greatest.

Let me illustrate. On the first day of November in the year 1700 there died of some obscure fever at his sumptuous domicile in Madrid a married Spaniard in the thirty-ninth year of his age, whose entire adult life had been passed in public office. These, I believe, are all the data that a Körösi would need to locate the deceased in that social group which should give full weight to the statistical significance of his death. A Ranke notes, however, that the death of Charles the Bewitched, neither entirely king nor completely imbecile, brought on the

War of the Spanish Succession, and that his reluctance to make a will betimes gave opportunity for intrigues which affected the European balance of power for a century thereafter.

The historian, then, cannot rely upon the statistical method, or upon any similar method, as a means of determining the significance of a specific event. No more can he, by the use of statistical laws, or by any other method of prognosis, undertake to tell what specific incidents of historical interest must happen. If prediction be the distinguishing mark of science, the historian must confess with humility, but let us hope also with tranquility, that he is not a scientific man. His method of explanation is a retrogressive analysis. Diagnosis, not prognosis is his art. As the physician applies his skill and experience to specific cases, not to categories, and is satisfied to determine what ailment produced the symptoms that he finds in his present patient, leaving the question quite open whether certain similar symptoms, in a patient of a different temperament, are due to the same or different disease, so the historian seeks to explain his events one by one, each as an individual case for itself. Laws, deduced by inference from other cases, are for him never demonstrative, but at most hueristic. They may serve to turn his attention to a probable cause but he will not be satisfied that it is the real cause until he has examined it individually. In this the method of history differs radically from the method of statistics. The method of statistics is, by consequence, only of indirect service to the historian.

May I sum up my conclusions so far by rewriting a statement of a former President of this Association? "The student of social science," said Colonel Wright,* "uses the results of statistical enquiry because he recognizes with the German Schlosser† that 'statistics is history ever advancing,' and that if he wishes to . . . keep himself fully and thoroughly informed of progress in every direction, he must use the statistical or historical method." The position which I have tried to present this morning would be better expressed by saying instead: The historian, as a student of social phenomena,

*Practical Sociology. 5th ed. 1904. P. 8.

†This apparent confusion between Friedrich Christoph Schlosser, the historian (1776–1861), and August Ludwig Schloeser, the statistician (1735–1809), appears in Edwards's address. Wright may have taken it from him.

uses the results of statistical enquiry whenever they appear to be to his purpose, just as he might use the results of any other science; for he recognizes that if he wishes fully and thoroughly to understand past progress in every direction, he must take all knowledge to be his province. But in using the results of statistical enquiry, he employs the historical and not the statistical method.

While, however, the historian uses statistics in the same manner as he uses the facts and theories of other sciences, the circumstance that the historical and the statistical methods both find their most fruitful application in the social field, enables the statistician to furnish historical data in a measure far more ample than will, say, the astronomer, or the chemist, or the embryologist. How frequently the historian shall find statistics among his sources will depend upon the direction to be taken by future applications of the statistical method on the one hand, and of the historical method on the other.

If, for example, Professor Muensterberg, having perfected his machinery for measuring the physical reactions of individuals in moral predicaments, shall furnish the world some day, with an accurate ethical calibration of the normal American or Teuton, the future historian will be greatly concerned, I am sure, to ascertain and to compare with the type, the reaction record of the fiftieth President of the United States and the tenth Emperor of Germany. You may regard the illustration as fantastic. But principles appear in extremes. What technical or financial obstacles meanwhile impede the collection of such statistics as the historian would like to use is not for him to say; but he may selfishly hope that the American Statistical Association will presently succeed in overcoming them all.

A change or extension in the subjects of history might also increase the availability of the data of statistics as materials for the historian. Freeman, for example, has declared that "history is past politics, politics is present history,"* and Seeley says that "it is with the origin and development of states that history deals."† Their dicta voice the fashion of their day. Other times, other manners. In the era of military feudalism, historians wrote chronicles of camp and court.

*E. A. Freeman. Lectures to American Audiences. 1882. P. 20.
†Sir J. R. Seeley. The Expansion of England. 1883. P. 148.

The Reformation obliged intelligent men to become more or less theologians, and from Luther to Voltaire ecclesiastical history predominated. The revolutions of the eighteenth century in America and France brought new subjects of historical study into vogue. Not only did Guizot and Hallam trace the constitutional development of France and of England, writing always with an eye upon the political situation in which they lived, but Grote constructed a ponderous History of Greece in support of the Victorian Whigs, and Mommsen launched a learned History of Rome against Napoleon the Second.

Observing thus how, with the shifting of contemporary interests in the past, the historical method has been applied (like the statistical) to widely diverse subjects, we are prepared to find, in our own time, that the increasing pressure of social and economic problems, into which, by the help of Darwin and of Marx, we seem to see more deeply than our fathers could, must in its turn induce the application of the old method to the new stuff of social and economic history. And since the subject matter of statistics is largely social and economic, the future historian, though working still chiefly in the old way, interesting himself, among other things in the personality of a leader of invention, a captain of industry, a freebooter among insurance companies and railways, or an organizer of international peace, may make, indeed he must make larger and larger use of the statistics that are and of the statistics that are to be.

Once more may I illustrate? It was long the habit of American historians to attribute our prosperity in 1789–1792 to the adoption of the Constitution in 1788. Of recent years, however, it has been more generally believed that, for reasons altogether independent of the Philadelphia Convention, the tide of prosperity was already swelling in 1787, and that the Constitution was borne to ratification upon its rising crest. This later view is, to my mind, not only the more probable inherently, but also the better supported by evidence; and much of that evidence is statistical. It is here characteristic, and further illustrative, that the new conclusion was not the immediate result of any statistical enquiry, but rather of a general judgment, such as historians are constantly pondering in their own minds, and then weighing and testing by all

appropriate means. In this case the chief means chanced to be statistical. And if, incidentally, some historian, in dealing with it, has fallen into a trap such as field errors in statistics leave ever open for the unwary, and, for example, has assumed to measure the growth of our foreign commerce by the use of figures which register, in part at least, merely the increasing efficiency of the newspapers in recording the entries and clearances of vessels, that blunder proves, to my mind, not that the true method for history is the statistical method, but merely that the historian needs, when using statistics as a source, just as he needs when using a scarab, or book of personal reminiscences, or a party platform as a source, to exercise an alert and competent criticism. However, the extent to which, being competent, he shall use statistical sources, or some other sort of sources, must depend chiefly upon the sort of things with which, as a historian, he shall chose to deal by his historical method.

It is even conceivable, to some minds, that the vogue in history may at length alter so completely as to eliminate altogether the element of personal interest, and that historians will some day deal solely with the social masses that statisticians have measured. General Walker will then appear more of a historian, even, through the footed columns of the Tenth Census than through the footnotes and pages of "The Second Army Corps," and a more enlightened age will find in the tables and diagrams of a municipal report greater historical eloquence than its forbears admired in Michélet. Possible these things are: to me they do not seem probable. And it is plain that before they shall come to pass, "history," as a term common to all the European languages, must have revolutionized its accepted meaning. Today, being no prophet, I have endeavored rather to deal with it in the meaning which it seems yet to possess.

One more comment and I am done. If any one of you is disposed to feel that even in their own way historians have made but grudging use of statistics, I beseech him to reflect, first, how difficult it is to get statistics read—not intelligently, but at all—and, second, how small is the fraction of historical time for which statistics give any appreciable variety of information.

THE SERVICE AND IMPORTANCE OF STATISTICS TO BIOLOGY.*

By Raymond Pearl, Ph.D., *Biologist, Maine Agricultural Experiment Station.*

The key note of anniversary celebrations is traditionally the contemplation of the achievements of the past. The American Statistical Association is today celebrating its seventy-fifth birthday. This for an individual is a very respectable old age, even in this day of lactic defenses against senility. In the life of a society, however, and especially of this Association, seventy-five years mark merely the beginning of life—the attainment of full youthful vigor—rather than the approaching dissolution of old age. Statistical science, which has been so ably promoted by this Association during the years of its existence, is still a growing and a developing science. By the discovery of new methods its power to solve problems is every day becoming greater, and every day new problems are being put before the statistician for solution by workers in other fields of science. Only a short time ago I was told by a distinguished biochemist that in his judgment many of the most fundamental problems relating to the chemical physiology of the animal body could only be solved by the application of statistical methods. Without these methods but little real progress on certain of these problems had been made. Great as have been the achievements of statistical science and of this Association in the past, one may well believe that they mark but the beginning, and will be far surpassed by new developments and applications of statistical methods of reasoning, in fields now either untouched or only just entered upon.

It is about one of these newer developments of statistical science that I have been asked to speak today. This is the application of statistical methods to the problems of biology. While relatively a new field, this branch of science has developed very rapidly during recent years. It has been given a special name, biometry, and a number of universities now offer

* Paper read at the seventy-fifth anniversary meeting of the American Statistical Association, Boston, Mass., February 14, 1914.

courses in the subject. A special journal—*Biometrika*—devoted to this field is now in its ninth volume. This occasion marks, I think, the first formal recognition of the existence of this youngest of its daughter sciences by the parent science of statistics. It is a great pleasure to assist at this "coming out party," in which I shall essay something like the part usually played by the débutante's frock, endeavoring modestly to draw attention to and set off her inherent charms and graces.

The application of statistical methods to the study of biological problems other than those of anthropology may be said to have begun with the work of the late Sir Francis Galton. Galton was a born statistician. He tells in his "Memories" of the instinct, which he inherited from his father, to arrange, classify and collect statistics about all sorts of things. At the same time he was deeply interested in problems of biology, particularly those having to do with inheritance. His interest in this direction crystallized into definite activity at about the time when his cousin Charles Darwin was elaborating his theory of heredity, which was called pangenesis. Galton instantly realized that this conception of the physiology of the hereditary process was essentially statistical in character, and that statistical methods were demanded to test and broaden it. Upon this work he therefore embarked with the vigor and ardent enthusiasm which characterized all of his scientific work. His results found expression in a series of memoirs and books which have become classics in biological science. Of these the most important is perhaps "Natural Inheritance," since in it are brought to a focus a number of different lines of work which engaged Galton's thought and energy for many years. In this book the attempt is made for the first time to determine, on a statistical basis, the degree of resemblance, in respect of bodily, mental, and temperamental traits which obtains between relatives of different degrees. Previously no attempt had been made to measure precisely these resemblances, which were, of course, a matter of common observation, though not of precise definition, to everyone.

In order to make the desired analysis of this problem it was necessary for Galton to devise new methods of dealing with statistics. The general mathematical foundations of statis-

tical science had, to be sure, been laid by Laplace and Gauss, and some progress in the application of these methods had been made by the man usually regarded as the founder of modern statistics, Quetelet, who was an honorary member of this Association in its early days. But none of these men had dealt specifically with the measurement of what are now known as correlated variations. From Galton's point of viewing the problem of heredity such a measure was an absolute necessity. He, therefore, devised one. It was not altogether a perfect one but was practically usable, and led very shortly to the application, by other hands, of an old theorem of Bravais, which furnished the entirely adequate measure which Galton had sought.

To the end of his life Sir Francis Galton retained his interest in the science of biometry, of which he may truly be said to have been the founder. His keenness of interest served in great part as the primal inspiration and stimulus which led two other distinguished English workers to enter this field and begin to rear the superstructure on the foundation already laid. I refer to Prof. Karl Pearson of University College and the late Prof. W. F. R. Weldon, whose untimely death took from English science one of her most brilliant intellects and charming personalities. To Professor Pearson belongs the very great credit of developing adequate and general mathematical methods for the analysis of biological statistics. Statistical mathematics in the main fall within the realm of the calculus of probability. The foundations of that calculus were laid by Laplace and Gauss, as has already been pointed out. Since their day the most notable fundamental advance in the mathematical theory of probability has, in the writer's judgment, been due to the genius of Karl Pearson. Until he began his work all statisticians, astronomers, and physicists who had anything to do with the theory of probability, either from the standpoint of statistics or that of the theory of errors of observation, had been content to use the so-called "normal" curve of errors to describe the distribution of chance determined events. One of the characteristics of this curve is that it is symmetrical. According to it events above the mean are as likely to happen as events below the mean. Observed statis-

tics of natural phenomena were found, as a matter of fact, to give in many cases asymmetrical distributions. Indeed, some of the very examples used in the text-books to illustrate the normal curve do not accord with it when tested by an accurate measure of goodness of fit (for which extremely valuable instrument of statistical research we are again indebted to Pearson). That convenient scape-goat "chance" was appealed to, however, to account for the discrepancies. Starting from the sound position that the facts of nature are of more importance than any theory, even though it be one beautiful enough to excite worship,* Pearson in three classic memoirs, in the series of "Mathematical Contributions to the Theory of Evolution," developed a theory of skew frequency curves, and skew correlation, which took account of the asymmetry so frequently seen in chance determined phenomena. This system of skew frequency curves has had the test now of nearly twenty years' usage. Every attempt at destructive criticism which has been aimed against it has failed. None of the substitutes, some of which have been proposed by very eminent mathematicians, have shown any approach to the generality and elegance of these curves.

Few biologists have an adequate conception of the extent to which biometry is indebted to Prof. Karl Pearson. If, as has been maintained, every real advance in science depends upon the discovery and perfection of a new technique, then, for whatever advance in biology may come through biometry, the debt to that distinguished investigator will be large for many years to come.

With so much by way of historical orientation we may now turn to a consideration of the actual concrete results which the biometrical method of attacking biological problems has yielded. Here, of course, only a few of the more important and outstanding points can be touched upon. Anything like a complete review of the pertinent literature is clearly out of the question in view of the fact that the statistical method is literally coming to permeate all fields of biological research. Starting, as we have seen, with the application to the problems of heredity, biometric methods first were taken into other

* Galton always insisted that the Greeks would have deified the "law of error" had they known of it.

phases of the evolution problem, as, for example, variation simple and correlated, natural selection, homogamy, etc. From these fields to comparative anatomy and taxonomy was an easy step. The whole field of experimental embryology and *Entwicklungsmechanik* clearly offered splendid opportunities for the biological statistician to bring his new technique into operation, which he was not slow to realize. In the field of animal behavior and comparative psychology statistical methods have taken a prominent place in the investigator's armamentarium, doubtless the more readily because of the considerable development of *Psychophysik* in the domain of human psychology. Finally, and most recently, biometric methods are making a place for themselves in cytology.

Out of such a wealth of material it is difficult to single particular researches for special mention. Instead let us try to examine in the brief time at our disposal some of the more noteworthy results of biometry in certain broad general lines. Foremost stands the subject of heredity. Here a curiously complicated condition of affairs has existed. At just the time (1900) when biometric researches in this field were being most actively prosecuted by Pearson and his co-workers came the rediscovery of the work of Gregor Mendel, and the laws of inheritance which that work established. It was at once perceived that these results gave a fundamentally new conception of the physiology of the hereditary process. This was, in certain respects, completely at variance with the concept of heredity developed by Darwin and Galton, which had furnished the leading idea in the investigations of Pearson. In the judgment of the great majority of the workers in this field, the newer Mendelian principles rendered the older methods of research essentially futile. It appeared that they were not adapted to get at the real kernel of the problem. This opinion has gained strength with the passing of time. It led, however, to the premature and unfortunate conclusion that, because biometric methods as applied to the problem of heredity in one particular case and one particular way did not lead to fruitful results, therefore these methods were of no use under any circumstances in dealing with this problem. Happily this view found an immediate corrective in the

investigations of the distinguished Danish botanist, Prof. W. Johannsen. His "*Elemente der exakten Erblichkeitslehre,*" of which a new and greatly enlarged edition is just off the press, gave a clear demonstration that biometric methods, when guided by sound biological ideas, were not only a great aid in the elucidation of the problems of heredity, but were in fact essentials. These methods enabled Johannsen to demonstrate with convincing clearness the fundamental distinction between phenotypic and genotypic variations; those, on the one hand, which have to do solely with the soma and not at all with inheritance, and those, on the other hand, which are inherited. This is a most fundamental concept, upon which all modern progress in the study of this great problem is based. Furthermore by the aid of biometric methods Johannsen was able to show the real nature and mode of operation of selection, artificial and natural. These results mark a real debt of biology to statistics.

It may fairly be said that the results which have been obtained by the application of statistical methods to biological problems in the field of *Entwicklungsmechanik* stands second in importance only to those in the fields of evolution and heredity. Biometry has furnished a valuable adjunct to the experimental method in the analysis of many problems of morphogenesis. In particular attention should be called to studies on growth, and also to studies on the general tectonic of the organism. By the application of appropriate biometric methods two fundamental laws of growth of wide generality in both the plant and animal kingdoms have been established. The first of these relates to absolute growth increments, and states that as an organism increases in size the absolute increment per unit of time becomes progressively smaller, in accordance with a logarithmic curve. In other words, the size which an organism has attained, after having grown a given time, is a logarithmic function, simple or complex as the case may be, of the time that it has been growing. The second law of growth, which, like the first, appears to be of wide generality, relates to the variability of the growing organisms, and states that relative variability tends to decrease progressively as growth continues. The problem of growth has been par-

ticularly studied from the biometrical standpoint, for some years past, by Dr. H. H. Donaldson and Dr. Shinkishi Hatai, of the Wistar Institute of Anatomy, using chiefly the white rat as material for investigation. The pioneer researches in this field were those of Prof. Charles Sedgwick Minot of Harvard Medical School, on guinea pigs. Other workers have studied with similar results the same problem in a number of plants.

Directly connected with the studies on growth has been the quantitative investigation of the principles of morphological organization. This I have referred to above as the problem of the general tectonic of the living body. This problem is one which has by no means as yet received the attention which is its due. It is one on which little real progress can be made except by the use of biometric methods.

Turning again to the general field of organic evolution a very important result of the statistical method has been the proof which it has given of the existence of assortative mating in the breeding of animals under natural conditions, and the measurement of the degree of this assortative mating or homogamy in particular cases. The significance of homogamy as a *vera causa* of evolution was first adequately emphasized by the post-Darwinians, and particularly by Romanes. He pointed out that: "Isolation takes rank with Heredity and Variability as one of the most fundamental principles of organic evolution. For, if these other two principles be granted, the whole theory of descent resolves itself into an inquiry touching the causes, forms and degrees of Homogamy." When this was written (1897) there was practically no definite and precise knowledge in even a single instance "touching the causes, forms and degrees of Homogamy." Since that time, as a result of the application of biometric methods to this problem, the degree of homogamy has been precisely measured in a number of cases, in some of which even its existence was not before suspected. Pearson and his co-workers have studied assortative mating in man. They have been able to show that for a wide variety of characters there is a definite and statistically significant degree of resemblance between husband and wife. Going to the other end of the

evolutionary scale it has been conclusively and independently demonstrated by Prof. H. S. Jennings of Johns Hopkins University, and the present speaker, that in the conjugation of Paramecium there is a high degree of assortative mating; "like pairs with like" to a marked degree. A rich harvest awaits further tilling of this field. Clear as is the significance of homogamy as a factor in evolution when viewed from the older standpoint emphasized by Romanes, the whole matter gains added significance in the light of the newer studies of heredity. The important investigations of Prof. E. M. East of Harvard University on heterozygosis clearly indicate, as a collateral issue, the significance of studying the degree of assortative mating which occurs in the reproduction of wild forms, for homogamy tends automatically to produce homozygosis. It may be said that we are not likely to gain any adequate conception or measure of the significance of heterozygosis as a factor in organic evolution until we have in hand the results of more extended investigations than have yet been made regarding the frequency of occurrence and degree of assortative mating in nature.

We may now turn in conclusion to the more general and philosophical aspects of the relation of statistical science to biology. As I have elsewhere pointed out, "the real purpose of biometry is the general quantification of biology. Its fundamental viewpoint is that without a study of the quantitative relations of biological phenomena in the widest sense it will never be possible to arrive at a full and adequate knowledge of those phenomena. This viewpoint insists that a description which says nothing about the magnitude of the thing described is not complete, but, on the contrary, lacks an element of primary importance. It insists, also, that an experiment which takes no account of the probable error of the results reached is inadequate and as likely as not to lead to incorrect conclusions."

Statistical science has brought to biology three fundamentally important things which it had previously lacked. These are: first, a method of describing a *group* of individuals in terms, not of its component individuals, but in terms of its (the group's) own attributes and qualities; second, the con-

cept of "probable error," which makes possible an estimate of the probable accuracy of a series of observations; and third, a method of measuring the degree of association or correlation between the variations in a series of characters or events. Time is lacking to develop each of these points, nor is it necessary, for they have been discussed in detail elsewhere.

In what has preceded I have tried to show that by turning to statistical science for aid the biologist has greatly augmented his powers of analysis in the domain of his own particular problems. While the branch of science, which has been called into being by this coalition, is yet too young to have shown its full capabilities, yet I think its achievements have been sufficient in quality and amount to justify the belief that its position is secure and its promise bright. Biometry seems destined to become a permanent and important branch, at once of biological investigation and of statistical inquiry.

THE TECHNIQUE OF PUBLIC STATISTICAL EXHIBITS.

By Charles J. Storey, *Supervisor of Exhibits, New York State Department of Health.*

The effective presentation of statistics in a public educational exhibit is fraught with great danger if the statistican exhibitor does not put himself in the public's place and design his exhibit from their standpoint rather than from his own. In fact, in more than one exhibit addressed to the poor of our cities and largely attended by them, statistics of a very complicated nature have been shown in an equally complicated manner; and although they, no doubt, occasioned curiosity and remark because of numerous brightly colored lines, they gave no instruction. The exhibitor should first of all study the viewpoint of the audience to be addressed. He must understand the mind of the public.

For an exhibit of statistical material, as for any exhibit, the preliminary plan should center around some real reason for the exhibit. This should be evident throughout the entire exhibit and should join the parts into one continuous whole. There should be a beginning, a climax, where a certain big idea should stand out, and an end.

The end of the exhibit should be as interesting as the beginning, and the climax, or the one thing emphasized above all others, should be so compelling that everyone who enters the exhibition will pass out with it in mind.

Preliminary plans of exhibits are usually good because it is much easier to outline an exhibit on paper than to transpose the same outline into exhibitable form. A recent exhibition had one of the best preliminary plans I have ever seen. It was orderly, in sequence, and complete, but the exhibit installed was just the reverse. In transposing from the written outline to the actual presentation and arrangement, someone had failed. The failure was, no doubt, due to lack of a good detailed working plan around which the exhibit is actually built.

An excellent way to see the exhibit as it will be when installed is to work what may be called *preliminary* filling cards on which every chart, placard, poster, etc. is described or duplicated, and indexed under its appropriate division. Nothing should be left to chance and every word and scheme of presentation should be under inspection and subject to revision as the preparation of the exhibit progresses. A final index of the material may be made when all the material is in and arranged. If this final index is made in duplicate, a good part of it may be used as a copy by the letterer or draftsman.

For a public educational exhibit, statistical information should be presented in the simplest possible manner. I have seen exhibits in the preparation of which the poor public was apparently never considered although the exhibits were avowedly made for them. Few people will bother to read many curves or to study out intricate charts. One of the best ways to get an idea of what the public will read or study is to scan the advertising pages of our good monthly magazines. The advertising man knows better than anyone else today just how much the public will read before the page is turned for something more interesting. He also knows what will attract and how to make it attractive. A telling advertisement does not require intensive study. It is prepared to compel attention and to force a certain idea upon the observer.

An exhibit should be entertaining. No matter how valuable the material presented is, if it is not entertaining, the points are lost. The motive back of an exhibit may be as serious as possible yet it must not be forgotten that the general public attends exhibits not to study but to be amused. The pill must be coated and the most clever exhibitor is he who does this best.

Simplicity and brevity are the keynotes of the successful exhibit. One should have much to select from but should select little. Too many exhibits have lost in effect by the quantity of material used. This is caused by the mistaken idea that the larger the exhibit the more successful it is.

Variety of display is far more important than variety of material. Poor material well displayed will teach more than the best ideas presented in a monotonous and unattractive

form. An exhibitor must have many methods of presentation. Uniformity of method is deadly. Everything in an exhibit should be designed in competition with the rest and everything should hit out and attract the observer.

An exhibit should be planned to show only the basic or principal points of the subject. One should not try to present all the arguments. A successful exhibit should be suggestive of the subject rather than complete. The main idea of an exhibit should be to make people think and to arouse interest for further study of the subjects presented. The ideas expressed should connote others. Detailed information may be exhibited in small form as in wall cabinets or albums which those interested may study but which the hurried or disinterested observer can pass by. Many exhibits are filled with inconsequential detail from the midst of which the vital points have a hard task to make themselves known. If an idea cannot be expressed in twenty words, it should be printed in a pamphlet and be given publicity that way.

Then again, big ideas are often too feebly expressed. Vigorous, terse expressions combined with size will put an idea to the forefront. A minute chart will not be noticed no matter how valuable it is. The size and arrangement of an idea to be expressed must be designed in relation to the surrounding exhibits.

The exhibit should have high lights or points of attraction. A certain uniformity of installation is desirable such as backgrounds and framing but variety of display is absolutely necessary. Moving exhibits attract more than anything else. Bright colors also invite attention and lend an attractiveness to an exhibit. White lettering on gray or black is hard on the eyes and not nearly as attractive as black letters on a gray or light buff back-ground. Roman or else block letters are the most readable. It is easier to read words composed of caps and lower case letters than words of all caps. The size of lettering must also be carefully considered and no letters should be used which cannot be easily read at least twenty feet away.

The statistician should not confine his means of expression to charts. Although graphic in form, if made for public

exhibition, they will usually be found too complicated, and a row of charts will drive most visitors away. This may be sad news for statisticians but it is, nevertheless, true. Only the very simplest curves should be use and these sparingly. One comparison on a chart is all that should be shown as more confuse. It must be remembered that most people read very few statistics in their day's work. If they can understand them, they find them too wearisome to concentrate their attention on them very long in an exhibit. Where there are simpler things to look at they go to them.

Comparisons by means of lines, either parallel or perpendicular, are easily grasped although, as mentioned, there should be only one point to be brought home. The comparisons should be plainly made and no complicated "keys" or systems of notation used.

Maps used to illustrate the geographical distribution of statistics should be utilized as sparingly as charts. In fact any one method of display cannot be employed very many times in an exhibit if the attractive value of it is to be retained. Where a map is used, it should be drawn so that everything is eliminated except the geographical data, such as boundaries and cities, necessary to present the facts or comparison. Showing statistics in a concrete form such as blocks of different sizes is an old and interesting method. Everyone remembers the exhibit of the number of tickets annually sold at subway stations, first shown at the New York Budget Exhibit. A map several feet long of Manhattan and the Bronx was used from which at the designated stations colored sticks of proportionate heights were placed.

To translate statistics into cartoons or posters is a popular and valuable method to bring home necessary information. For instance, one cartoon seen recently pictured a waterfall, the flow of water illustrating the enormous infant mortality, which when compared to the number born (contents of the reservoir) was extremely effective. The figures and percents were lettered in their respective places and the whole taught a lesson which no amount of unaided statistics could have done. When comparisons are made, appeals to past experi-

ences of the observer should be made; that is by the use of homely illustrations.

A striking method of "coating the pill" is to combine statistics with a few words of text. These figures should be round numbers wherever possible as they are more easily and quickly read. Most persons mentally reduce a large number to its nearest thousands or hundreds as the case may be. For the same reason, unless the number or percent is very small, not more than two decimal places should be used and preferably none at all. Cents should be left out of items whenever possible as they serve only to confuse.

In an exhibit where one idea or item is considered many times, a helpful way is to always put the figures or symbol in a distinctive color. This will aid the observer to recognize the figure at once and it will also serve as a starting point for his interest.

THE INFLUENCE OF MARRIAGE ON THE DEATH-RATE OF MEN AND WOMEN.

By George I. Bliss, A. B., AA. A.

Prof. Walter F. Willcox, in his sixth annual report to the New York State Commissioner of Health, submitted some very interesting tables as to the influence of marriage on the health of men and women. His study was based on the population and deaths of New York state outside of greater New York and Buffalo for 1909–1911. Prof. Willcox showed that the death-rate of husbands is much lower than that of bachelors at each age group, except 80 and over; and that at the ages from 30–49 the death-rate of the former is less than one half of that of the latter. He likewise showed that the death-rate of married women is lower than that of spinsters at each decennial age group, excepting 20–29, as it is set forth in the following table:

TABLE I.

POPULATION, DEATHS AND DEATH-RATES CLASSIFIED BY SEX, AGE, AND MARITAL CONDITION, NEW YORK STATE (EXCLUSIVE OF NEW YORK CITY AND BUFFALO).

(Average of 1909, 1910, 1911.)

MALE.

Age Period.	Single.			Married.		
	Population.	Deaths.	Death-rate.	Population.	Deaths.	Death-rate.
20–29	231,625	1,532	6.6	128,339	533	4.2
30–39	76,614	986	12.9	229,128	1,344	5.9
40–49	37,172	724	19.5	196,465	1,856	9.5
50–59	21,142	607	28.7	144,972	2,466	17.0
60–69	9,881	504	51.0	98,673	3,151	31.9
70–79	3,759	381	101.4	40,015	2,910	72.7
80 and over	754	154	204.2	6,134	1,258	205.1

TABLE II.

FEMALE.

Age Period.	Single.			Married.		
	Population.	Deaths.	Death-rate.	Population.	Deaths.	Death-rate.
20–29	145,537	684	4.7	176,583	1,006	5.7
30–39	54,769	405	7.4	227,473	1,428	6.3
40–49	35,812	357	10.0	187,947	1,536	8.2
50–59	19,389	386	19.9	124,673	1,803	14.4
60–69	12,062	448	37.1	68,781	1,933	28.1
70–79	5,623	462	82.2	21,112	1,296	61.4
80 and over	1,058	296	279.8	1,848	360	194.8

The results were so startling that they aroused a good deal of comment in the daily papers, and merit further investigation. But in view of the dearth of reliable statistics on this study in the United States I resorted to the well-known letters of Dr. James Stark to the Registrar-General of births, deaths, and marriages in Scotland, and found very interesting tables on this subject.

I therefore submit the following table showing the population and deaths of married and unmarried men at each quinquennial age group and the death-rate per 1,000 living:

FROM THE NINTH DETAILED REPORT (1863).

TABLE III.

MARRIED AND UNMARRIED MEN IN SCOTLAND.

Ages.	Married.			Unmarried.		
	Number Living.	Deaths.	Death-rate.	Number Living.	Deaths.	Death-rate.
20–25	22,946	137	6.0	106,587	1,251	11.7
25–30	54,221	469	8.7	48,618	666	13.7
30–35	66,153	600	9.1	25,962	383	14.8
35–40	63,858	690	10.8	15,857	253	16.0
40–45	62,645	782	12.5	12,311	208	16.9
45–50	54,505	869	15.9	8,824	179	20.3
50–55	49,591	880	17.7	7,636	205	26.8
55–60	38,006	929	24.4	5,550	142	25.6
60–65	35,920	1,216	33.9	5,242	227	43.3
65–70	22,021	1,134	51.5	2,848	156	54.8
70–75	16,029	1,291	80.6	2,021	205	101.4
75–80	9,716	1,135	116.8	1,081	157	145.4
80–85	5,477	953	174.0	513	101	196.9
85–90	1,708	488	285.7	151	32	211.9
90–95	449	137	305.1	50	21	420.0
95–100	103	40	388.4	6	3	500.0
100 and above	28	15	535.7	3	.....	.....
All ages	503,376	11,765	23.4	243,259	4,189	17.2

The first striking fact which this table reveals is that the death-rate of the bachelors was double that of the married men between the ages of 20 and 25. As its persons became older, this excessive difference in the death-rates of the married and unmarried decreased slowly and regularly, showing the difference in favor of the married men at every period of life. It is thus proved that the state of bachelorhood is more destructive to life than the most unwholesome trades. When we come to the total death-rate at all ages, however, the very reverse is the case. The general death-rate among married men is very much higher than that among single men; so that, while only 1,723 bachelors died during the year out of every 100,000 bachelors, 2,338 married men died out of a like number of married men.

This apparent contradiction may be explained as due to the fact that the number of bachelors being far greatest at that period of life when the mortality is very low, namely, from 20 to 24, whereas the number of married men is greatest at those periods of life when the mortality is high, seeing that the mortality increases with age. Furthermore, almost half of all the deaths of the bachelors occur before the thirtieth anniversary, at which period the mortality is much lower than at the more advanced periods of life. When the whole deaths at all ages are thrown together and compared with the total bachelors living, the general mortality seems to be little higher than that due to the earlier period of life. Among the married men, on the other hand, the greatest number of deaths occurs between the sixtieth and seventy-fifth year of life, at which period the mortality is high as compared with the number living. Consequently, when the total deaths of the husbands of all ages are compared with the total living, a high mortality seems to have prevailed, because the persons were all so much older when they died than were the bachelors. Therefore, comparing the total deaths of the married at all ages with the total deaths of the bachelors, necessarily leads to a false conclusion. In comparing mortality rates of two or more classes, to be correct, it must be limited to comparing at each age group, and the smaller we take the age group the more nearly correct are the rates.

Before we proceed to compare the facts relative to the very different death-rates of the married and unmarried men in the whole population of Scotland for the year 1863 with those brought out by Professor Willcox we must bear in mind that the widowed and divorced were included among the married men in the former, while in the latter they were tabulated separately. According to Professor Willcox the death-rate of widowers and divorced men is very much higher than that of husbands of the same age and about the same as bachelors between the ages 30 and 80. Making due allowance for the widowed and divorced men Table I is in every point corroborated by Table III. Had Professor Willcox tabulated his statistics in quinquennial ages he would, doubtless, have found the relative difference in the death-rate between husbands and bachelors higher between 20 and 29 and also higher for ages 80 and over.

In view of the striking difference between the death-rates of married and unmarried men shown to exist at each quinquennial age in the population of Scotland for the year 1863, it was decided to investigate the subject further. Accordingly another table was prepared based on the statistics of 1864. It is not necessary to give the table here but I might mention the fact that it corroborated in every point the preceding one.

The different death-rates prevailing among married and single men in Tables I and III were brought out by a comparison of the deaths with the living, where the correctness of the conclusion rests on the truthfulness of the numbers of the married and unmarried as ascertained at the census. But the correctness of the conclusion may be tested by estimating the mean age at death of the married and the unmarried, and if the two results corroborate each other, we may place greater confidence in the results deduced from the facts.

I have calculated the mean age at death of all the husbands and bachelors from 20 to the close of life and find that the mean age at death of husbands in Professor Willcox's table was 60.0, whereas the mean age at death of the bachelors was only 43.6, a difference of 16.4 years. In Table III the mean age at death of the married was 59.7, whereas the mean age at death of the bachelors was 40 years, a difference of 19.7 years.

It would appear that the husbands in New York State (outside of New York City and Buffalo) have a chance of living 16.4 years longer than those who remain bachelors; and that the married men of Scotland have a chance of living 19.7 years longer than the unmarried.

These statistics seem to prove that the married state is best fitted for mankind. The married man is, in general, not only more healthy, vigorous, and free from the diseases of the unmarried, but he is also more regular in his habits, is better housed, better fed, and better attended to. The married men may be accounted as selected lives; for the weak, the delicate, those suffering from disease of any kind, the dissipated and the licentious are not as likely to marry, so that a larger proportion of such are found among the ranks of the unmarried men; and these are known to die at a much higher rate than the robust and temperate. This is the natural explanation of the difference which exists in the death-rates of the married and unmarried.

The various reasons enumerated above, however, would only account for the difference in the death-rate of the married and unmarried during the earlier years of life, say from 20 to 40 years of age, but quite fails to explain the difference in the death-rate at the higher ages, for almost all those who remain unmarried from natural infirmity of constitution, or from being addicted to intemperate or licentious habits, die before they have attained their fortieth anniversary. Very few survive the critical age of 50. Therefore, all bachelors who survive their fortieth year, and certainly all those above 50 years of age, must be considered as selected lives; and the difference found to exist in the death-rates of the married and unmarried in that period of life can reasonably be attributed to causes connected with their being married or unmarried.

In the seventh and eighth detailed annual report (relating to the year 1861 and 1862 in Scotland), attention was directed to the influence of marriage on the death-rate of women. Two tables were published, based upon the return of 1861 and 1862, showing the different death-rates which prevailed at each quinquennial period of life of those who were married, and those

who were unmarried. The widows and divorced women were included among the married.

The following table shows the number of the living, and deaths of the married and unmarried women at different ages, in Scotland in 1862, and the death-rates per 1,000 living at each age group:

TABLE IV.

MARRIED AND UNMARRIED WOMEN IN SCOTLAND IN 1862.

Ages.	Married.			Spinsters.		
	Number Living.	Deaths.	Death-rate.	Number Living.	Deaths.	Death-rate.
20–25	39,924	364	9.1	114,180	895	7.8
25–30	71,649	683	9.5	61,044	506	8.3
30–35	77,503	764	9.9	34,954	367	10.5
35–40	71,962	806	11.2	24,368	300	12.3
40–45	69,660	806	11.7	19,690	225	11.4
45–50	58,934	785	13.3	14,659	222	15.1
50–55	54,479	826	15.2	13,973	269	19.3
55–60	41,462	886	21.4	10,216	244	23.9
60–65	42,390	1,200	28.3	11,405	370	32.4
65–70	26,242	1,242	47.3	6,875	350	50.9
70–75	20,337	1,342	65.5	5,544	434	78.3
75–80	12,190	1,424	116.8	3,230	359	111.1
80–85	7,490	1,278	170.6	1,986	332	167.2
85–90	2,646	708	267.6	582	161	276.6
90–95	763	282	369.7	181	55	303.9
95–100	203	94	463.1	48	20	416.7
100 and above	34	17	500.0	23	3	130.4
All ages	597,868	13,507	22.6	322,958	5,112	15.6

Comparing the mortality of the married and unmarried women at every quinquennial age group, it is seen that the high death-rate of the married women appears to be confined to the ages under 30 years; but that from 30 to 39 years, the death-rate of the married women falls below that of the spinsters. At the ages 40 to 44, the mortality of the married again slightly exceeds that of the unmarried. From age 45 to old age, however, the married die in smaller proportion than the unmarried.

The results brought out by this table as to the difference in the relative mortality between married women and spinsters are practically the same as by Professor Willcox's table excepting at the ages 40 to 44, and again at the extreme ages 75 and upwards. In the former table the mortality of the married, at ages 40 to 44, slightly exceeds that of the spinsters,

whereas in the latter table the mortality of the spinsters exceeds that of the married. Had Professor Willcox, however, given the death-rate by quinquennial age-groups, it would probably have shown the same break, for if we combine the two quinquenniums in the former we find a higher death-rate for the spinsters. At the extreme ages again the higher death-rate shown among married women than among spinsters is due to the fact that widows and divorced women were included in the former table and the high death-rate was probably caused by widows, who generally predominate at this period of life.

It appears that the higher death-rate suffered by the married women below the thirtieth year of life is probably caused by the greater danger to life which attends the bearing of the first child, for at this age period a large proportion of married women give birth to their first child. But when that age was attained when the great majority of married women had survived the birth of their first child, namely, above 30, the mortality of the married women fell below that of the unmarried. From 40 to 45 when the usual "change of life" occurs, the mortality of the married again exceeds slightly that of the unmarried. But from 45 to old age, the death-rate of the married is distinctly better than that of the spinsters. Above age 75 the relative mortality fluctuates, but the numbers are too few to draw any conclusions.

It is worthy of consideration how far the higher rate of mortality shown to exist among spinsters above age 30 may be due to extensive understatement of their ages. If the spinsters were on the average three years older than they represented themselves to be in the census returns, and the ages of the married women were correctly given, this would probably fully account for the greater apparent mortality of the spinsters.

The relative mortality of spinsters and married women among insured had been recently investigated by the Medico-Actuarial Mortality Investigation Committee, based upon the experience of forty-three life insurance companies (M. A. M. I. Vol. II, page 36). It showed a distinctly lower mortality for spinsters than for married women, excluding widows and divorced women, at every age group, the average difference

for all ages being 38 per cent. in favor of spinsters. The favorable mortality of the spinsters as compared with the married women deduced from insurance statistics, however, is not a characteristic of the class in the general population. The great majority of spinsters insured by the companies were teachers, stenographers, librarians, and other similarly favorable types. Only a small proportion of them were employed as saleswomen in the stores, or were engaged in factory work. Therefore, these statistics of the insured women do not disprove the conclusions reached by Professor Willcox and fully corroborated by the tables from Scotland, that the influence of marriage on the death-rate of the female sex, though not so potent as on the male, is considerable.

THE NEED OF A FEDERAL TRADE CENSUS.*

By Melvin T. Copeland, *Instructor in Commercial Organization, Harvard University.*

The study of domestic trade in the United States is seriously hindered by the dearth of readily accessible information. To facilitate researches in this field there is a need of a federal census of market distributors—dealers in raw materials, commission merchants, wholesalers, and retailers. These merchants make up a numerous and, in general, a necessary class in our population. Their activities affect every inhabitant of the country. There would be a widespread and vital interest, therefore, in the results of a census which would show how many people are gaining their livelihood from trade, the number and size of stores of the various classes, their geographical distribution, and the amount of their business.

Such trade statistics would give enlightenment upon subjects of general and local concern. What is the ratio of retail stores to population? Is the total number or the number of any class of stores increasing relatively faster than population, or is it relatively decreasing? We can only guess. Some cities are primarily jobbing centers; their manufactures are comparatively unimportant. But through a change in freight rates or for other causes their trade and their prosperity may be slipping away. In other cities the jobbing trade is rapidly expanding. But all the estimates of wholesale or retail trade now made are apt to be biased or inaccurate and are altogether inadequate. Reliable information concerning the volume of trade and the agencies through which it is carried on can be obtained only by a federal census.

The costs of market distribution are by no means light. Many commodities of every day use such as cotton cloth, clothing, shoes, and groceries, ordinarily yield the wholesaler and retailer together a total gross profit of at least 30 to 40 per cent. of the retail price. Although, as a rule, the wholesale and retail trades are not great fortune makers, the total cost

* Paper read at the quarterly meeting of the American Statistical Association, Boston, Mass., December 9, 1913.

of distributing finished products to the consumers not infrequently exceeds the amount paid for the raw material plus the costs of manufacturing. With a view to economy attempts are continually being made to shorten the route from the factory to the consumer. Traditional market methods are upset. A life and death conflict ensues. Twenty years ago most of the shoes manufactured in the United States were handled by jobbers; now the proportion is, perhaps, one-half and shoe manufacturers have even gone so far as to operate their own retail stores. Textile manufacturers have been dispensing with the services of commission agents. Some manufacturers of goods retailed through grocery stores no longer sell to wholesalers but direct to retailers. "Trusts," have in some instances either ceased to sell to jobbers or have at least restricted the amount of trade handled by that class of middlemen. It was the jobber and the retailer, not the consumer, who felt oppressed by the old American Tobacco Company.

New types of retailers—department stores, chain stores, coöperative societies, and mail-order houses—have, on the other hand, become such large scale purchasers that they have demanded and received jobber's discounts. As a consequence they can undersell the small independent retailer and threaten his very existence. Before this bitter strife reaches its climax a demand may be heard for restrictive legislation, such as the department-store taxes of the German states. In fact some attempts have already been made to secure the passage of a law prohibiting the giving of rebate coupons in the retail tobacco trade. The question of price maintenance, the right of the manufacturer to compel observance of a fixed retail price, has not been settled by the recent decision of the Supreme Court in the Sanatogen case. The issue will attract more attention from our legislators in the future than it has attracted in the past. One of the most forceful arguments in favor of price maintenance is that it tends to protect the smaller retailer against the price-cutting policy of the department stores and the chain stores. The results of these changes in marketing methods can, at the present time, be judged only in a general way. We have no reliable statistical information to show how far the changes have gone nor what the actual tendencies

are. By giving us facts a federal trade census would enable a more intelligent consideration of the questions of public policy involved.

In order to analyze their markets, manufacturers and large mercantile houses have sought statistics of the number of wholesale and retail stores which could serve as outlets for their goods. To meet this demand lists have been prepared by several agencies, samples of which are given in the table below. Although in several instances these lists correspond with each other fairly closely, they are, in the main, widely divergent. List A was published in the *Mahin Advertising Data Book,* 1913; List B in *Printer's Ink,* January 18, 1912; and List C in *Advertising and Selling,* April, 1912.

NUMBER OF RETAIL STORES.

Territory.	Drugs.			Hardware.			Jewelry.		
	List A.	List B.	List C.	List A.	List B.	List C.	List A.	List B.	List C.
United States ..	43,588	45,176	37,074	30,504	39,446	27,270	21,684	20,145	18,249
New York.....	2,828	3,965	1,830	2,005	2,636	1,430	1,544	2,132	1,320
Pennsylvania...	2,342	3,315	2,450	1,576	2,393	1,480	1,405	1,702	1,470
Ohio.....	1,469	2,099	2,052	2,037	2,288	1,770	1,245	1,163	1,230
Illinois.....	1,766	2,833	1,787	1,967	2,995	3,320	1,152	1,431	1,101
Missouri.....	2,518	2,407	2,370	1,367	1,932	1,215	908	836	670
Ma ne.....	407	406	*	263	402	*	250	253	*
Georgia	991	1,041	*	313	536	*	309	262	*

* Data not given.

These are all reputable publications which would not willingly, I think, circulate inaccurate statements. The figures given here for three trades in seven states are typical in indicating the discrepancies which exist between all such figures. As a brief examination will show the variations are not uniform. The list with the largest total for the United States does not give the largest number for each of the individual states; note the figures for drug stores in Missouri and for jewelry stores in New York, Pennsylvania, and Illinois. And the list with the smallest total for the United States sometimes has the largest number for an individual state; note the figures for hardware stores in Illinois. Although doubtless due in part to variations in definition, the discrepancies throughout are irreconcilable. This is sufficient answer to the argument that the collection of such information should be

left to private investigations. In a federal census the definitions would be standardized and the statistics more accurate. The information thus provided would be of distinct help to business men.

The only attempt at an official collection of such trade statistics was made in the Massachusetts census of 1905. The schedule was simple but included several of the questions which should be asked in the proposed federal census. No serious difficulties were experienced in the collection of the information. Nevertheless the results as published have little worth. Largely because of the urgency of the work on the population and manufacturing censuses, which had the right of way over the trade census, the classification and tabulation of the trade statistics were not satisfactorily carried out. This was shown most strikingly in an incongruous combination of wholesale and retail figures. Despite its non-success, however, the experiment does not indicate that there are any insuperable obstacles in the way of a federal trade census; on the contrary, it makes the proposal appear more practicable.

It is to be frankly recognized that, as in other census work, nice problems of definition and classification will be encountered. There will be omissions and duplications, as in the manufacturing census. In the Abstract of the Thirteenth Census (p. 514) it is stated that: "The figures for some industries do not represent the total production, because important establishments that manufacture the same class of products may be included in other industries." Other qualifications are appreciated by all who have had occasion to analyze carefully the census statistics. Yet the results are generally accepted as approximately correct, and the percentage of error would probably not be higher in the trade census. In filling out some of the schedules for the trade census it might be necessary to accept estimates, but it is doubtful if this would occur as frequently as in the agricultural census.

Under present conditions and with the type of enumerators which must be employed, it would be a hopeless task to try to secure statistics of costs of store operation. The accounting systems of large establishments are diverse, and many small stores have only a haphazard system of bookkeeping or none at

all. The Bureau of Business Research of the Harvard Graduate School of Business Administration found it essential to prepare a uniform system of accounts before it could obtain comparable figures for the costs of retailing shoes. Hence, whatever the federal government may eventually do in this line of investigation, no question about costs should be included in the schedule for the first trade census.

The following questions seem to be the most important of those practicable to ask:

1. Kind of business carried on.
2. State whether specialty retail store, department store, general store, coöperative store, mail-order house, wholesaler, jobber, commission merchant.
3. Number of stores operated (*i. e.*, branches).
4. What proportion of goods sold is manufactured by proprietor.
5. Actual capital, owned and borrowed, invested in the business.
6. Net cost of goods sold during year.
7. Total amount of sales during year.
8. Number of employees:
   Male, 16 years of age and over.
   Female, 16 years of age and over.
   Children.
   Total.

The results of the questions concerning the age and sex of employees would shed light upon the labor conditions in certain classes of stores. In addition some inquiry concerning wages ought to be made, the more comprehensive the better. Unless preliminary investigation proved it to be inexpedient, it is suggested that informaton be collected from department stores and general stores as to the amount of their sales of each of several groups of commodities, such as footwear, dry goods, hardware, furniture, and groceries and provisions. It would be inadvisable to undertake to follow out this line of inquiry in all its possible ramifications but for the broader classes significant statistics could probably be obtained.

As to the date of such a census, it was at first proposed that

it should be taken in 1915, in connection with the census of manufactures. But in view of the comparatively short time which would then be allowed for planning the schedule and also because of the delay in tabulation which might result from pressure of work on the manufacturing statistics, it would, perhaps, be more feasible to take the trade census at a date intermediary between 1915 and 1920, say 1917. This would relieve somewhat the fluctuations in the burden placed upon the census bureau.

Practically all of the arguments in favor of a census of agriculture or manufactures apply also to the proposal for a trade census, particularly since this trade census does not appear to involve more serious statistical problems nor a prohibitive cost. The number of persons engaged in trade is so great, their relation to our whole economic life is so intimate, the costs of marketing are so high, and such fundamental changes are taking place in market organization that we need reliable trade statistics which can be secured only through a federal census.

THE NUMBER OF JEWS IN NEW YORK CITY.

By Henry Chalmers, *Cornell University.*

The increasing prominence of the Jew in the manifold activities of the metropolis gives interest to the repeated estimates as to the number of Jews in New York City. For some time the figure has been roughly given in articles and addresses by public men as "about one million"; but seldom has any attempt been made at accuracy or any definite basis been given for the varying figures. The United States census not being permitted to make inquiry of individuals as to their religion, no census estimate is available. In the census of 1910, for the first time, the question of "What is your mother tongue?" was asked. It appears that in New York City 861,980 persons reported Yiddish or Hebrew as their mother tongue. But this is undoubtedly an underestimate, as many Jews born in Germany, for instance, consider German their native tongue; moreover, Jews of the second generation who have become Americanized are more apt to report English than Yiddish as their native tongue.

The only recent efforts at exact estimate of the number of Jews in New York City, to my knowledge, are those of Dr. J. Jacobs, editor of the *American Hebrew,* and Dr. Walter Laidlaw, secretary of the New York Federation of Churches, both made early in 1912. Dr. Jacobs* estimates the number at 906,000 on January 1, 1912; while Dr. Laidlaw† finds it to be 1,265,000 on April 15, 1910, which, when carried forward to January 1, 1912, by his method, reaches 1,365,000. In view of the divergence between the two, the larger exceeding the smaller by 50 per cent., I have attempted to analyze their methods and reconstruct the estimate on a better statistical basis.

Dr. Laidlaw's estimate is based on a house-to-house racial and religious census of certain sections of New York City conducted by the New York Federation of Churches at various dates since 1895. Ten years ago he computed the percentage

*New York Times, January 27, 1912, page 4.
†New York Times, February 10, 1912, page 10.

of the various nationalities whose families reported themselves as Catholic, Protestant, and Jewish and published the following distribution of the population of New York as in 1900:

	Number.	Per Cent.
Roman Catholics,	1,266,561	37
Protestants,	1,572,629	46
Jews,	598,012	17
Total	3,437,202	100

Applying to the population of New York in 1910 the same percentages as were applied in 1902, he estimates the Jewish population on April 15, 1910, at 1,265,000. Carrying out his rate of increase for the decade, I get for January 1, 1912, an approximate total of 1,365,000.

Dr. Jacobs, of whose first estimate Dr. Laidlaw's figures were a criticism, replied in a later issue of the *New York Times,** declaring he was unconvinced by the Federation's computation for these reasons:

"First—If Dr. Laidlaw's figures were correct, the Jewish death-rate of the city would be 8.25 per 1,000, an impossible figure. (The death-rate for the general population in 1910 was 16 per 1,000.) Again, it would work out a marriage rate of 6.25, when that of the city was 9.66.

"Second—Dr. Laidlaw's inquiries seem to have extended over the last fifteen years, during which the marked characteristic of the Jewish population has been its mobility. It is likely enough, therefore, that his inquirers have frequently met with the same families and counted them over and over again.

"Third—Jewesses have larger families than others, while, on the other hand, there is a larger proportion of single young men among the Jewish population of the city. These facts would disturb Dr. Laidlaw's percentages based, as I understand, on the nationalities of Jewish mothers, and

"Fourth—Dr. Laidlaw's researches apply mainly to the congested districts, where the Jews mostly congregate. Obviously there is likely to be a larger proportion of Jewish

*New York Times, February 12, 1912, page 10.

families in the tenement districts than in the 'Silk Stocking' districts. Hence, his results are sure to exaggerate the size of the Jewish element when applying the percentages derived from congested sections to the census figures of the whole city."

In addition to these obvious criticisms of Dr. Jacobs, the essential weakness of his method, to my mind, is that it lacks the essence of a census count—a detailed photograph of the population as it stood at a certain point of time. A long-continued exposure of a moving object is apt to give a very confused and inexact negative. While interesting as a sociological study of the religious make-up of certain sections of New York City, the Federation's count extended over the fifteen years between 1895 and 1910, during which, as Dr. Jacobs suggests, "the marked characteristic of the Jewish population of the city was its extreme mobility." They have moved uptown and across the bridges, absorbing vast numbers of newcomers and losing old members. Not only is it probable that the same family has been met and counted more than once, but that others were omitted altogether. Moreover, the Jewish population of the city is marked by an unusually large number of young men and women who are here without any family, and so the Federation's basis of the nationality of the mother of the family could take these into account, if at all, but very roughly. To assume that in 1910 the three main religious divisions—Catholic, Protestant, and Jewish—claimed the same proportion of the total population as the Federation estimated in 1900 is to take no account of the vast changes continually going on in the make-up of a seething center and port of entry like New York City.

In view of these weaknesses in method, it is hardly to be expected that Dr. Laidlaw's estimate of 1,265,000 in 1910 and 1,365,000 in 1912 would be anything but a crude approximation.

Dr. Jacobs's first estimate of the number of Jews in New York was made in 1902. Starting with the number of Jewish deaths reported in 1901, he computed the number of Jews at that time as 600,000, which the *New York Times* declares was generally accepted, though at first considered surprisingly

large. He obtained it by assuming that, while the death-rate of the whole city was 20, that of the Jewish element was 14 per 1,000, "which I fixed upon for various good reasons." He assigns no further reason for assuming a death-rate 30 per cent. smaller than that of the general population except to suggest that the Jewish element contained a larger proportion of persons in the healthy ages. The number of Jewish burials increased 38 per cent. from 1901 to 1910. While the general death-rate of the city decreased some 23 per cent. in that period, Dr. Jacobs assumed that the Jewish death-rate decreased only 3.5 per cent., or from 14 to 13.5 per 1,000. Upon that basis, of 13.5 deaths per 1,000 population, he gets an average Jewish population during 1910 of 820,000. Adding to that the number gained by immigration and excess of births over deaths from July 1, 1910, to January 1, 1912, he arrives at a total of 906,400 as the Jewish population of New York City in 1912.

Dr. Jacobs's study, while proceeding on apparently good statistical bases—the death-rate among the Jews as checked up by the marriage-rate and the yearly immigration figures—does not carry out its methods completely and involves a number of untested assumptions. Moreover, it arrives at an estimate hardly probable and not consistent with the changes in the Jewish population during the decade, as we know them.

A total of 906,400 for the number of Jews in New York City in 1912 is hardly plausible and well merits the epithet of the *New York Times* as "surprisingly small." It will be recalled that the "mother tongue" inquiry of two years earlier, 1910, found 861,980 who reported Yiddish or Hebrew—and that clearly underestimated the actual number of Jews. Moreover, in the decade from 1900 to 1910, the proportion of total foreign-born in the city increased over half, or 52.7 per cent. Dr. Jacobs's estimate would show an increase for the Jewish element of only 42 per cent.—clearly impossible, when we consider that the number of those born in Russia and Austria-Hungary, the lands that furnished eleven twelfths of all the Jews arriving from 1884 to 1905, increased in the same decade 145 per cent., or almost three times as fast as did the general foreign-born.

Of course, we have no record as to how many of those born in Russia and Austria-Hungary and now living in New York City are of Semitic stock, or of some branch of the Slavic peoples. But isn't it fair to assume, considering that race persecution has been one of the greatest incentives for the emigration of Jews from these lands, that the Semites have formed as large a proportion of the Russians and Austro-Hungarians as the Slavs of the total numbers coming to the port of New York in the last decade? Not to content myself with so broad an assumption, I have attempted a closer approximation from the records of the Annual Report of the Commissioner-General of Immigration as to the immigrants for the fiscal year ending June 30, 1911, "By States of Intended Residence and Race or People." Of those apparently natives of either of these two countries who declared their intention of making New York state their residence, about 47,000 were Hebrews; while, adding up the total numbers of Russians, Poles, Lithuanians, Croatians, Ruthenians, Magyars, Slovaks, and others hailing from either of the countries considered, gave a total of non-Hebrews of 51,000.

Considering that the Jew is more apt than the Slav to stay in New York City instead of seeking other sections of the state, I am fairly well supported in my assumption that, of the total additions to the population of New York City from Russia and Austria-Hungary in the last decade, the Jews probably constituted as large a proportion as the Slavic folk. Since these two countries increased their numbers in the city by 145 per cent. in the decade, does it seem that 100 per cent. for the Jewish element would be too great? That would bring up the number of Jews in New York in 1912 to somewhere between 1,100,000 to 1,200,000. Is it reasonable to assume that, while the city as a whole increased 39 per cent. in the decade, the Jewish population that had received so phenomonal an influx of new blood has increased no faster (42 per cent.), as Dr. Jacobs would have us believe?

Let us carry out Dr. Jacobs's own method more closely than he does. The death-rate of New York City decreased in the decade about 23 per cent. Assuming that the Jewish element, especially with the addition of so many in the healthy ages,

shared in the general improvement, the number of deaths per thousand should fall from 14, which Dr. Jacobs assumes for 1901, to 10.8 in 1910. This would give us an average population during the year of 1,022,700—instead of Dr. Jacobs's 825,000 for 1910. Adding his estimated increase through natural and migratory causes, of 88,100 between July 1, 1910, to January 1, 1912, we arrive by his own method at a total of 1,100,800 on January 1, 1912.

But, to my mind, the crux of the whole problem lies in the validity of the basic assumption that in 1901 the Jewish death-rate was 14 per 1,000, or 30 per cent. below that of the general population. Dr. Jacobs offers no more evidence than to say "which I fixed upon for a number of good reasons." I believe he is right in using the death-rate as probably the best basis for estimating the Jewish population, but I see no reason for assuming 14 per 1,000 in 1901 and 13.5 in 1910. I have, therefore, tried to reconstruct his estimate upon tested assumptions.

Selecting four wards of Manhattan where the Jewish element clearly predominate—the seventh, tenth, eleventh and thirteenth—all south of 14th Street and east of the Bowery, I tried to ascertain the death-rates in these wards as a clue to the probable death-rate of the Jews in the city as a whole. In 1900 the death-rates were: seventh ward, 17.3 per 1,000; tenth ward, 16.6; eleventh ward, 16.3; thirteenth ward, 14.6. Dividing the total deaths in the four wards by their combined population, I find the death-rate of the most typically Jewish section of the city to have been 16.3 per 1,000. I believe that represents very closely the death-rate of the whole Jewish population. It surely is no lower, for, if the Jews uptown and across the bridges in Brooklyn and Queens live under more healthful conditions than their brothers in these congested sections, any observer will testify to the fact that the lower East Side region, covered by wards selected, contains a larger proportion of persons in the healthy ages, recently arrived young men and women without families.

Comparing 16.3 for 1900 with the mortality of the same wards in 1910, obtained through the kindness of Dr. W. H.

Guilfoy of the New York City Department of Health, I find them as follows:

Seventh ward,	12.8
Tenth ward,	11.0
Eleventh ward,	8.6
Thirteenth ward,	9.2
Average for the four wards,	10.3

Assuming, similarly, that these wards represent approximately the mortality of the Jewish element in the whole city—say 10.5 per 1,000—my estimate of the Jews in New York City on July 1, 1910, would be close to 1,050,000. Adding to that Dr. Jacobs's estimated increase from July 1, 1910, to January 1, 1912, by net immigration and excess of births over deaths, I arrive at a total of 1,140,000 for the Jewish population of New York City on January 1, 1912—a figure neither as low as Dr. Jacobs's 906,000, nor as excessive as Dr. Laidlaw's 1,365,000.

With these two points fixed—the Jewish mortality in 1900 at 16.3 and in 1900 at 10.5—I worked backward and found that, assuming a progressive improvement in the death-rate during the decade, the rate for 1901, the year of Dr. Jacobs's first estimate, was 15.8 per 1,000. The number of Jewish deaths in that year was 11,026, which would give an average population during the year 1901 of 506, 300, or at most 510,000. It will be remembered that both Dr. Jacobs and Dr. Laidlaw agreed upon 600,000 as the Jewish population in 1901, a figure that the *New York Times* describes as "at first considered surprisingly large."

It will thus be seen that Dr. Jacobs's estimate involved a twofold error: first, in assuming for 1901 a population clearly too large by 90,000; and, secondly, in assuming 14 per 1,000 as the death-rate in 1901 and 13.5 in 1910, when a closer and more accurate study shows them to have been nearer to 15.8 and 10.5, respectively.

I prefer my own estimate of 1,140,000 for January 1, 1912, for two reasons: first, it is based upon a direct study of four typically Jewish sections of the city, surely a more reliable

basis than a guess at a death-rate; and, secondly, it shows an increase during the decade more consistent with the general increased longevity of the city and the great influx by immigration. While the death-rate of the city as a whole improved some 23 per cent. in the decade, Dr. Jacobs's estimate would reduce the Jewish mortality only 3½ per cent., in spite of the addition of persons mainly in the healthy ages. My estimate would point to a more probable and reasonable decrease in the death-rate of 34 per cent. Again, with the number of those born in Russia and Austria-Hungary—the native lands of eleven twelfths of the Jews in New York—increasing 145 per cent. in the decade, it surely is more plausible to believe that the Jewish population increased 110 per cent. in the decade, as my estimate would imply, than to accept Dr. Jacobs's increase of 42 per cent.

Attempting to bring the estimate up to 1914, it appears that the last published mortality records of New York City, by wards, are those for 1911. I have therefore resorted to the less exact, though only available, basis for estimate. In the period from July 1, 1901, to January 1, 1912, the Jewish element increased on the average 8 per cent. annually. Assuming this progressive rate of increase to have continued since, the number of Jews in New York City would rise from the 1,140,000 we obtained for January 1, 1912, to a total of 1,330,000 for January 1, 1914. I would advance the last figure, however, only as a probable approximation, until the bases for a more exact estimate become available.

REVIEWS AND NOTES.

The Influence of Monarchs. By F. A. Woods, The Macmillan Company, N. Y. 1913. $2.00.

Most scientific work necessarily consists in the slow and patient process of adding stone to stone. New foundations are put in adjacent to the old walls and it is hard to see where the work of one builder begins and that of the last leaves off.

Now and then, however, a bold spirit leaves the old structure entirely, steps out into the unknown, and traces the outlines of a new building. These are the works of real originality amid a host of routine investigations. To produce one of them is a rare achievement but Frederick Adams Woods has given us two such works within a decade. His Mental and Moral Heredity in Royalty (1906) stands alone as a comprehensive study of the inheritance of mental and moral traits; and the volume just published on The Influence of Monarchs is even more unique, since it represents the first attempt at an introduction of quantitative methods into the broader field of the history of nations.

The method which Dr. Woods has christened Historiometry consists in subjecting the facts of history "to statistical analysis by some more or less objective method." He points out in the present work how the writing of history has in the past been dominated by the subjective personal opinion of the historians and its general conclusions, therefore, failed of that "concerted agreement" which, as he says, is "the business of science." No one presumably will doubt the value of an objective statistical analysis if the data warrant it. The obvious criticism of such a method is that the raw materials upon which it works in the case of history can, in many instances, only be human opinions and these opinions might be considered too inaccurate to be capable of systematic statistical treatment. The peculiar service rendered by Dr. Woods is that, in his several investigations, he has made the experiment and obtained positive and definite results which enable him to conclude that "the judgments of historians are now proved and known to be a fair approximation to the truth." He has shown that these judgments are, within limits, reliable and that demonstrable objective conclusions may be drawn from them by the application of statistical methods.

The plan of the present work is a simple one, though like all statistical studies it must have involved very considerable labor. The material consists of the history of the fourteen principal nations of Europe for a period extending from the definite beginning of national history (between the years 987 and 1,525 in different instances) to about the end of the eighteenth century. In all 368 different reigns, regencies, or other governmental periods in these different countries form the units of study. In each case the general condition of the country is graded in three

different grades, + or − or ± according as the country showed an advance in economic and political status, a decline or a doubtful condition. The ability of the ruler for each period is graded on a similar scale. In each case the grades are assigned, not on the personal judgment of the author, but solely from an objective comparison of the statements made by a dozen historical authorities chosen beforehand for each country.

The bulk of the book consists of a summary of the material in the form of brief histories of the fourteen countries and their rulers, preceded by an Introductory chapter and by one on the Philosophy of History and Historiometry. The historical chapters are excellently done and seem to the layman to be worthy additions to history in the conventional sense. The Interpretation of the Results and a concluding chapter on Causation in History are the ones which concern us more nearly, however, from the statistical and scientific point of view.

Three hundred fifty-four cases out of the 368 studied are analyzed in the final tables. In 105 instances a superior ruler was associated with an advancing period, in 31 cases both ruler and period were doubtful, in 87 cases a weak ruler and a period of decline were associated, giving 223 cases or 63 per cent. showing complete identity. In only 11 cases was a strong ruler associated with national decline and in only 30 cases was a weak ruler associated with prosperity, giving 41 cases or less than 12 per cent. of direct conflict. The correlation between the ability of the ruler and the condition of the country works out at about .65 with a probable error of .05.

It is evident that this correlation, as compared with others which have been derived from a study of biological phenomena, is a very large one and must be considered as highly significant. The correlation in physical structure between brothers is only .50; between parents and offspring is only .40. It cannot be discredited by criticism of the sources from which the gradings were originally drawn. Their errors of judgment, if they were random errors, would only tend to lessen the correlation figure and the fact that it is obtained in spite of them shows that the records of the historians chosen must have been reasonably in accord with underlying facts. The only thing which could vitiate the results would be a constant bias, a systematic policy of magnifying the prosperity of a country under a strong monarch or of glorifying the personal ability of a monarch whose realm enjoyed unusual prosperity. Criticism on this score does not, however, seem to be justified, when one studies with care the data as to individual reigns and rulers and notes how clear and definite their characteristics are apt to be and how conservative Dr. Woods has been in assigning doubtful cases to the intermediate class.

A correlation between able rulers and prosperous national conditions might of course be due either to an influence of monarch upon national life or to an influence of national life upon the personality of the ruler. Dr. Woods advances evidence of three kinds for the former rather than the latter view. First, the transition from advance to decline, or the reverse follows in case after case immediately upon the death of a ruler.

Secondly, conditions were generally bad during regencies, or interregnums. Of such periods tabulated 36 were classed as −, and 12 as ± and 18 as +, most of the latter being associated with the personalities of powerful non-royal regents. Finally, Dr. Woods has shown in his earlier book on Mental and Moral Heredity that the personal ability of the members of the same royal families here studied, including those who did not reach the throne, was in entire accord with their ancestry and was apparently uninfluenced by their environment. The conclusion seems, therefore, clear that it was the sovereigns who controlled history, not the reverse. It should be noted that of the fourteen countries studied there is one (and only one)—which fails to show a positive correlation between ruler and national life. This is England where between 1603 and 1811 there were 10 reigns, every one marked by advancing conditions while there was only one ruler of eminence three of the intermediate and six of the minus grade. This fact suggests the warning that what was true for continental Europe in the period studied may be modified by other forces, such as we may surmise have been at work since the industrial revolution. This is only a surmise, however, like all other personal surmises, about historical causation. Dr. Woods has for the first time substituted for surmise a convincing quantitative study of the weight of one single factor, the ability of rulers, in the development of nations.

It is likely that Dr. Woods' book will only by degrees gain full recognition in the circles where it should have its most important influence. Work which lies in the beaten track of fashionable research is quickly taken advantage of but the attack on really new problems with new tools, as in the case of Mendel's discoveries, is only slowly comprehended. This is particularly the case here where the whole point of view which Dr. Woods has carried over from Biology will be strange and unfamiliar to the historian whom his work most concerns. The members of the American Statistical Association, with their scientific training on the one hand and their direct contact with the social sciences on the other, may have an unusual opportunity to interpret this notable book and the novel and fruitful methods which it exemplifies to the more orthodox historian.

C.-E. A. Winslow.

Money and Prices: A Statistical Study of Price Movements. A dissertation submitted to the faculty of the graduate school of arts and literature of the University of Chicago in candidacy for the degree of doctor of philosophy. By James Dysart Magee. Chicago: The University of Chicago. Reprinted, with additions, from the Journal of Political Economy, Vol. XXI, Nos. 8 and 9. 1913. Pp. 89.

Money and Prices is an inconclusive study based upon unreliable statistical methods.

In the words of the author the object of the study is to test "the relationship which exists between certain price changes and certain changes in amounts of money. Then the question arises of how this narrower problem is related to the discussion over the validity of the Quantity Theory of Money, which is the principal point of difference in the discussions of momentary theory at the present time. . . . Perhaps the opponents of the Quantity Theory of Money are talking about what actually happens and the adherents merely about a hypothetical state of affairs. Our problem then may be stated in alternative ways: we are attempting to find out how far the alleged proportionality does occur; or, we are attempting to find out to what extent 'other things' are not 'equal' in actual experience." (Pp. 11, 12.) In solving this problem Dr. Magee makes three classes of investigation as follows: "First a study is made of the correspondence of certain particular price movements in given markets with the movement of money or deposits in banks or money in circulation. . . . The second type of investigation concerns the relation of movements in the amount of money to the movements of wholesale prices. . . . The third type of investigation examines certain attempts at statistical proofs of the Quantity Theory of Money. These proofs make allowances for the factors omitted from what we have called the crude theory." (Pp. 12–15.)

The statistical method used by Dr. Magee in making his tests was explained in his article on "The degree of correspondence between two series of index numbers" in the QUARTERLY PUBLICATIONS for June, 1912. In that article he objects to the use of the Pearsonian coefficient of correlation as a means of testing the relationship between two series of index numbers because (1) "it entirely disregards the element of time which in most problems in which index numbers are used, is of prime importance"; and (2) "a given quantity may be added to or subtracted from the terms of our series without changing the value of the correlation coefficient."

The first objection does not hold if we use Mr. R. H. Hooker's method of computing the correlation coefficient from the "differences between successive values of the two variables." ("On the correlation of successive observations," Journ. Roy. Stat. Soc., Vol. 68, p. 697; see also the reviewer's "The correlation of economic statistics," QUARTERLY PUBLICATIONS OF THE AMERICAN STATISTICAL ASSOCIATION, December, 1910.) Con-

cerning this method Mr. Hooker says that "correlation of the difference between successive values will probably prove most useful where the similarity of the shorter rapid changes (with no apparent periodicity) are the subject of the investigation, or where the normal level of one or both series of observations does not remain constant." (*Ibid.*, p. 703.) G. Udny Yule uses Hooker's method of differences in measuring the correlation between the annual infantile mortality per 1,000 births and general mortality per 1,000 living. (See Yule's Theory of Statistics, p. 197.) The method of differences accomplishes two results: (1) it reduces, although it does not entirely eliminate, the influence of the growth element on the coefficient; and (2) it has the effect of making the *order in time* of the items of significance.

Dr. Magee's second objection to the coefficient of correlation as stated above calls attention to a virtue, not a defect, of the coefficient. That "the correlation coefficient of two series of index numbers is not changed if we add a constant to each term of one series" is true. But that this fact "is perhaps the strongest argument against using it for testing the relationship between two sets of index numbers" is decidedly untrue. Suppose that we are using the ordinary graphic method of comparing the fluctuations of two series of index numbers, each series represented by a graph with time as abscissas. If the ordinates are such that one graph is so far below the other that comparison is difficult we shift the former upward or the latter downward for convenience in comparing the fluctuations. This graphic process is identical with the algebraic process of adding or subtracting a constant to or from, as the case may be, each term of the series. It may be said, indeed, that any method of measuring correlation which does not have the mathematical property to which Dr. Magee objects is erroneous.

As a substitute for the coefficient of correlation the author suggests that the two series be treated as follows: First, multiply each item of one of the two series by such a multiplier as to make the initial items of the two series identical. "Next," he says, "we may get the amount of change in each case and take as the degree of correspondence the fraction which has the smaller change for the numerator and the larger change for the denominator. . . . By treating each successive change in this manner and then taking the arithmetic mean of the results we obtain what may be called the Degree of Correspondence. . . ." Thus, in comparing the two series: 2, 4, 6, 8, 10 and 5, 10, 15, 20, 25, Dr. Magee would multiply the items of the first series by 5-2, obtaining a series identical (in this case) with the second series. For this pair of series the fractions are all 1 and the arithmetic mean of the fractions, or the "Degree of Correspondence," is therefore 1.

The author uses this method throughout his investigation to test the relationship between weekly, monthly, and yearly, (1) price indices for bonds, stocks, farm products, certain speculative commodities, and wholesale prices of commodities on the one hand, and (2) specie and legal tenders in the New York clearing house banks, net deposits, and per

capita and total circulation on the other hand. Finally, he applies the same method to the figures obtained by Kemmerer and Fisher as his (Dr. Magee's) test of the truth of the quantity theory of prices. As a matter of fact, however, in more than nine tenths of the cases the author was contented to take +1 in case the changes of the corresponding items were in the same direction, and —1 in case they were in opposite directions, giving the arithmetic average of the algebraic sum as his "Degree of Correlation." Out of some 114 tests the resulting average was below 0.10 in 46 cases, from 0.10 to 0.19 in 42 cases, from 0.20 to 0.49 in 21 cases, and from 0.50 up in 5 cases. *It is noteworthy that all of the five cases of a high "Degree of Correspondence" came from comparing yearly figures where the growth element predominates.* In the words of the author "the correspondence is greater between price movements and the movement of money or deposits in banks or of money in circulation when the averages used cover longer periods than when they cover shorter periods." (P. 54.) He does not appear to recognize, however, that like growth elements in the two series make his "Degree of Correspondence" unreliable as a measure of the synchronization of fluctuations. (See the table given later.)

The application of the test to Kemmerer's and Fisher's figures leads Dr. Magee to the conclusion that "in neither case is much correspondence shown for direction and amount of movement; our inference is, then, that these two attempted proofs of the Quantity Theory of Money are far from being conclusive." (P. 49.) Before taking the author's word for the non-conclusive character of the Kemmerer and Fisher investigations let us see how his method works in sample cases.

Suppose the method be applied to the following pair of series:

100,	150,	200,	250,	300
100,	200,	300,	400,	500

Both series are arithmetic progressions and move in the same direction. The "Degree of Correspondence" is ½. Now consider the pair of series below:

100,	101,	102,	103,	104,	105
100,	110,	120,	130,	140,	150

The "Degree of Correspondence" is but 1-10, although the items move together perfectly, *i. e.*, for an increase of one unit in the first series there is an increase of ten units in the second series. Likewise consider the following pair:

100,	101,	102,	101,	103,	100
100,	105,	110,	105,	115,	100

These series fluctuate together perfectly, *i. e.*, for a change of one unit in the first series the second changes five units in the same direction. Yet Dr. Magee's "Degree of Correspondence" for the pair is only 1-5. It is evident that any coefficient is possible for perfect correspondence. Dr. Magee's entire study is based upon this erratic coefficient.

The reviewer will add the following table:

Magee's method of "Degree of Correspondence" applied to a pair of series having the same direction of growth element, *e. g.*, series giving the relative per capita money in circulation on July 1, and the relative per capita consumption of malt liquors in the United States, 1890–1908.

(Statistical Abstract of the United States, 1909, Folder p. 19.)

Year.	Index of money in circulation.*	Index of consumption of malt liquors.†	Correspondence.		
			+	−	0
1890	90	90			
1891	93	97	.43		
1892	97	100	.75		
1893	95	106		.33	
1894	97	101		.40	
1895	92	99	.40		
1896	84	104		.62	
1897	90	98		1.00	
1898	99	105	.78		
1899	101	101		.50	
1900	106	105	.80		
1901	111	106	.20		
1902	112	115	.11		
1903	116	118	.75		
1904	122	120	.33		
1905	123	121	1.00		
1906	127	132	.36		
1907	127	139			0
1908	137	138		.10	
Totals			+5.91	−2.95	0

* Base, 1890–'99. All items multiplied by 0.9375, making the initial item identical with that of the second series.

† Base, 1890–'99.

"Degree of Correspondence," 1890–1908=(5.91—2.95)÷18=0.16.

"Degree of Correspondence," 1899–1908=(3.55—0.60)÷10=0.295.

"Degree of Correspondence," considering only the direction of the change for the period 1899–1908=(7—2+0)÷10=0.50.

For the ten years, 1899–1908, the "Degree of Correspondence," as Dr. Magee would figure it, between per capita money in circulation and per capita consumption of malt liquors is very high, 0.50. Does this indicate that the circulation is increased by drinking alcoholic beverages, or *vice versa?*

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URUGUAYAN STATISTICAL REFORM.

[American Minister Nicolay A. Grevstad, Montevideo.]

The General Council of Statistics has taken steps to bring about an important reform of the commercial statistics of the Republic. Hitherto the import and export values have been based upon prices fixed, in many instances years ago, for fiscal purposes. These prices no longer represent the real value of the articles imported or exported; they are either too high or too low, generally too low. The council intends to base the statistics, beginning with the current year, upon the real values, as near as possible, and to that end solicits the coöperation of leading importers and exporters. This reform is held to be necessary in order that Uruguay may, in the comparative commercial statistics of the world, occupy the place to which it is justly entitled by the volume and value of its commerce.

F. L. H.